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ORIGINAL LECTURES.

A CASE OF SUPPOSED PROGRESSIVE PERNICIOUS ANÆMIA.

*An Abstract of a Clinical Lecture delivered in the
Philadelphia Hospital.*

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GENTLEMEN: The history of this case is as follows: Mary L., æt. 47, has been from her youth subject to irregular menstruation and menorrhagia, and yet until three years ago she never suffered from any marked attack of illness. At that time she was admitted to the Episcopal Hospital to be treated for a metrorrhagia, and she partially recovered after six months' treatment. Her health, however, continued to decline, and a year ago she had presumably an attack of malarial fever, from which she convalesced, but her general health failing, she was admitted to this hospital during last fall. It is proper to add that she has led a fast life, and a specific taint may be suspected, though it cannot be established. Her condition has not materially changed since her admission, and with these data I commenced the investigation of her case February 1st, when I assumed the care of the wards. It is obvious that intense anæmia is present. The tongue, the mucous membrane, and the skin, all present an extremely blanched appearance, the latter is also of a yellowish hue. Digestion is impaired, and the motions of the bowels irregular. Physical diagnosis reveals that the splenic dulness is increased, the spleen measuring four inches transversely, by five in the vertical direction. Over the heart there is a bruit which, upon the usual inferential reasons, is probably functional or hæmic. The examination of the retina is negative. A diagnosis in this case can be most certainly reached by exclusion. The enlarged spleen, the blanched appearance of the patient, and the progressive character of the symptoms suggest leucocythæmia, but since the disease has existed for three years, the possibility of an incipient splenic leucocythæmia antedating a leucocytal excess, cannot be entertained. In leucocythæmia enumeration by the hæmacytometer should show not only the white corpuscles increased, but the red corpuscles diminished. Usually when the disease is established the proportion of white to red is greater than 1 to 20. For example, instead of 500,000 red corpuscles per cubic millimetre, there may be only half so many or less; and the white may be increased from 15,000, the normal average, up to several hundred thousand per cubic millimetre. The red corpuscles are usually normal in appearance, sometimes unduly pale; the leucocytes are often both larger and smaller than normal, and reagents develop several nuclei. No such increase has been found in this case, consequently further investigation must be made.

Hodgkin's disease is a process which may be defined

as a more or less widely spread enlargement of the lymphatic glands, accompanied frequently by enlargement of the spleen and by progressive anæmia. The pathological feature of this process, viz., the lymphatic hyperplasia, is absent, so this disease must be laid aside.

The question of progressive pernicious anæmia in this case has been one of deep interest to me, and is my reason for bringing the case before you. The pathology of this disease is obscure, lesions in the medullæ of the bones, the spleen, and the supra-renal capsules, have been described, but its course is indicated by the terms pernicious, malignant, progressive, idiopathic.

The blood lesion in this case has been studied for me by Dr. Wm. E. Hughes, Medical Registrar of the University Hospital, with the hæmacytometer with the following report: "600,000 red and 3000 white corpuscles to the cubic millimetre. That is, the red are reduced to about one-ninth of their normal number, while the white are very slightly decreased, less than one-half. The red corpuscles are, most of them, larger than normal, some as large as white corpuscles; a few of them are very small. They are well colored, but some are tailed and otherwise irregularly shaped. The white are probably a trifle increased in size." This appearance of the blood is believed to accord with the lesions of this tissue in pernicious anæmia. In an idiopathic anæmia the appearance of the red corpuscles would not be so ill-shaped; they would be paler than normal, and also smaller. The exact changes of the blood in what may be termed symptomatic anæmia, such as in malaria, syphilis, tuberculosis, or albumenoid disease, etc., cannot be said to be positively established. Dr. Hughes, in his note, observes that in his experience in malarial anæmia the number of red cells is above 1,500,000, and the white not less than 50,000. Syphilitic anæmia would present the same characteristics as malarial anæmia with respect to red cells, but the white would be about 5000 in the cubic millimetre. Dr. Hughes has examined the blood from a large number of cases, and his observations are entitled to attention.

I believe, however, that deductions from physical signs should be made only after an analysis of the sum of the clinical history and the symptoms. The malarial element must be excluded from this case because the attack occurred only a year since, and the symptoms of this patient antedate this time. The syphilitic hypothesis is also insufficient because there are not distinct evidences of syphilitic lesions in the past or present history of the case. The circumstance that this patient always suffered from irregular menstruation and hemorrhages at the monthly periods, is of consequence, and it seems to me that she may suffer from a form of anæmia allied to the chlorotic. In other words, the depreciation of the patient's nervous vitality united to the history of menorrhagia has exercised a specially depreciating influence upon the blood-making function, which has resulted in a reduced elaboration of the corpuscular elements of the blood. It is, however, possible that the malaria has acted as an accelerating causative

complication, and perhaps this attack of malaria may account for the enlargement of the spleen.

With the above views, viz.: that in this case the excessive blood destruction depends upon insufficient activity of the blood-making functions in the economy, the line of treatment should be purely alterative and stimulant to the nutritive functions, and also tend to supply the essential elements of nutrition.

As an alterative, I believe no remedy will prove more signally useful than the continued use of mercury in some form. Small doses of bichloride of mercury, ranging between the $\frac{1}{60}$ th and the $\frac{1}{100}$ th of a grain, so that without salivation the stimulant influence of this drug as an alterative tonic may be kept up. Small doses of calomel, $\frac{1}{60}$ th to $\frac{1}{100}$ th of a grain, are sometimes equally useful. This treatment must be maintained for many weeks, or even months, but after a six weeks' course of the mercurial, iron, which previously may not have produced any good effect, can be used with advantage. While using the mercurial I shall order small doses of Fowler's solution at meal-time, and the mercurial in the intervals, three times daily. Occasional laxatives or digestive stimulants, such as strychnine or pepsine, form suitable adjuvants to this plan of treatment. To supply the essential elements of nutrition I shall order a liniment, composed as follows:

R.—Lin. Saponis,
Ol. Morrhuæ, aa \mathfrak{z} vi.—M.

A tablespoonful or more shall be rubbed into the thighs and abdomen night and morning.

Much careful thought should be bestowed on diet. I shall recommend the Gluten Food, prepared by the American Health Food Company, and Murdock's Liquid Beef, as especially useful.

NOTE (March 28, 1884).—At this date the patient's health is obviously improved. There is a tinge of red around the lips and tongue, and the strength is increased. Dr. Hughes reports that the examination of the blood showed 1,305,000 red corpuscles, which is a very decided improvement on the condition when first examined. The size and shape of the corpuscles, however, are very little changed, most of them being larger than normal, and many of them still irregularly shaped.

ORIGINAL ARTICLES.

REMOVAL OF FOREIGN BODIES FROM THE AUDITORY CANAL.

BY H. B. HEMENWAY, A.M., M.D.,
OF KALAMAZOO, MICH.

ABOUT four years ago, one Monday morning, I met an old acquaintance and successful surgeon at the depot of a small village in Illinois. He asked me if I could meet him that morning, to assist him in a small, but bothersome, case. He gave me the following history. I made no records of the case, except in my mind, and, hence, must relate the events from memory.

The evening before (Sunday), Mrs. T., the wife of a lake captain, brought her little boy, about five years old, to the office of Dr. B., to have him remove "something from his ear." She said he had

been playing that afternoon with the other children. In his play, he got a "lemon-stone" into his ear, and could not get it out. It gave him but little pain, but affected his hearing. Upon examination, the doctor could easily see the stone, not very far back, probably at the most contracted portion of the canal. He placed the child's head on one side, and tried to dislodge the stone by jarring. It did not come. He still regarded it as a simple case, and took his forceps; but, in trying to get hold of the stone, he pushed it further in. Then, though several times he seemed to have a good hold, the stone would always slip from his grasp just as he thought it was coming out. He tried the use of wire and silk snares, but with the same result. He tried to hold it by tenacula, but could not. He worked until his patience was about used up, if his patient was not, and the ear was in a worse condition than when he began. He then told the mother to take the boy home, and he would come around the next morning, and give him chloroform. He explained that he could see better in the daytime, and that the pain in the ear was so great that the little fellow would resist his efforts, unless he should be under the influence of an anæsthetic.

Meanwhile, the doctor endeavored to invent some new method to try. He searched his books for some suggestion, but to no purpose. After his relation of the above history, remembering a hint which my old professor of anatomy, Dr. R. L. Rea, had given us as to the shape of the auditory canal, I asked the doctor if he had tried an ear-spoon. He replied "no;" he had none, and if he had, he could not see how that would help the matter very much. I explained the *rationale*, and offered the spoon from my pocket-case.

We found the patient in a very good condition. There was considerable inflammatory action, and, hence, sensitiveness and pain in the auditory canal. With the spoon, we could feel the stone, and found it now impacted, the swelling of the tissues helping to keep it firmly in place. Equal parts of chloroform and ether were given. After working some time, without making any progress, the doctor was about ready to give up the attempt, when I managed to turn the stone over. Slowly, but surely, we then drew it along the canal. My ear-spoon is of the common style—ear-spoon and groove-director combined. After the stone was turned, our chief trouble lay not so much in the slipping of the spoon from the stone, as in the fact that we could not get a firm enough hold on the spoon with our fingers. We need a cross-bar or loops for the fingers on the handle. When, finally, we brought the stone to the concha, we found that it was not a lemon-seed, but a cherry-stone.

I have given the history of this case thus fully, for it well illustrates how much trouble one may have with a very simple matter. With an ear-spoon in the first place, Dr. B. could easily have removed the stone in a few minutes, but, after the swelling took place, it was very difficult.

What is the *rationale* of the removal of foreign bodies from the auditory canal? The canal extends from the concha, inwards, forwards, and downwards,

being slightly bent, the convexity of the curve being upwards and backwards. The canal has its smallest diameter near its centre. A cross-section of the canal shows it to be ovoidal in shape, the longest diameter being vertical, except at the inner end, where it is nearly horizontal. The foreign body is likely to lodge in the contracted portion of the canal. The use of forceps (which, with a general practitioner, is the first instrument tried in such cases) is almost sure to push the body back beyond the constriction, and thereby increase the difficulty. If used at all, the operator must remember that there is more room above and below than at the side of the foreign body. An ear-spoon, however, can almost always be passed over the body far enough to get a good hold. The spoon is so shaped that it takes very little more room than the foreign body, when applied like the blade of the obstetric forceps. When the body lies against the membrana tympani, remember that there is more room in front, or behind, than above the body. It is better to pass the spoon behind the body; for, if passed in front, the pressure upon the body would tend to push it through the membrana tympani. On account of the shape of the canal, too, the foreign body is more likely to lie against the anterior than the posterior wall of the canal. Inasmuch as the surgeon will want his instrument above the foreign body, when it nears the contracted portion of the passage, it will generally be best to pass it neither directly over, nor exactly behind, but above and behind the object to be removed. In this position, on account of the relation of the parts, the tip of the spoon will simply be passed along the wall of the canal and the membrana tympani, until in position for traction. In the case related above, the instrument was easily passed into position, and but for the partly macerated and badly swollen condition of the parts, extraction would not have been very difficult. It was the most difficult case of the kind I have ever seen, and well illustrates the fact that a surgeon had better leave a case entirely alone, unless he is fully prepared to treat it.

Other modes of extraction. Forceps may do very well for many cases, but in those in which the doctor is the most perplexed, they generally avail but little. Of course, the foreign-body forceps of the aural surgeon are to be preferred to any other form; but few general practitioners possess such an instrument, and even if they did, in many cases it would be found useless. A snare oftentimes may be of great assistance; but it is very difficult to make it stay on a hard, smooth, round body like a cherry-stone. The syringe will frequently remove objects that have not passed the constricted portion, and may assist in the removal of small objects from the vicinity of the membrana. When used upon a large object that is located beyond the constriction, and which nearly fills the canal, it only macerates the tissues.

While in other modes of manipulation, the position of the head is important, it is especially so in the use of the syringe. With the head of the patient thrown a little back and toward the affected side, if the operator, pulling the pinna auris upward, and a little backward, throws a *gentle, steady* stream of *lukewarm* water along the superior wall of the canal,

the momentum of the current reflected from the membrana tympani against the mass, together with the action of gravity, will quickly accomplish the desired result, if this method will succeed at all. Much force must not be used.

A little hot wax or glue on the end of a stick has been recommended, if the body has not passed the constriction. It seems to me that this is to be condemned as being less efficient and more dangerous than any of the other methods. In some cases, the foreign body may be dissected and removed in pieces. If, however, the object is as hard as a cherry-stone, it would probably be less harmful to leave it in than to attempt to break it. In a few cases, a tenaculum may be used, but care must be taken that harm be not done by its slipping.

SEROUS CYSTIC TUMORS OF THE ORBIT.¹

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In vol. xii. No. 2, *Archives of Ophthalmology*, Dr. J. L. Thompson described one case of cystic tumor of the orbit, removed by electrolysis. I wish to place on record four cases treated by a different method, resulting in permanent relief.

CASE I.—W. M., aged eighteen years, consulted me relative to a small growth projecting from the left upper eyelid, situated below the orbital ridge to the nasal side. Vision, both eyes = $\frac{20}{L}$; refraction

myopic. No lesion in fundus oculi. Mobility of eye not interfered with. The elevation was round, glistening, and translucent; the skin over it quite thin, somewhat tense; it had attained the size of an almond. As its contour could be changed by manipulation, I suspected a sac filled with fluid, which was confirmed by making an examination with an exploring needle. The fluid escaping was clear yellowish, of about the consistency of honey. The tumor was detected when the patient was five years of age.

CASE II.—Miss B., aged fifteen years, a well-developed girl, was brought to me in October, 1881, by her mother, with the following history: When the patient was six years old the mother detected a small, soft growth, about the size of a pea, under the skin of the right upper eyelid, nasal side. The family physician was consulted. He advised non-surgical interference at this time. The tumor has slowly increased in size; barely discernible to an observer's eye, but distinctly felt by fingers. There has been no pain. The exploring needle revealed fluid. No lesion in fundus oculi. Vision, both eyes = $\frac{20}{XX}$. Family history irrelevant.

CASE III.—James C., aged twenty-four years. At the age of ten years, a small tumor was noticed below the nasal end of the eyebrow, about the size of a bean. The patient was seen by me, for the first

¹ Read before the Philadelphia Ophthalmic and Aural Book Club Association, February 18, 1884.

time, in April, 1882. The tumor at this time was about the size of a shellbark, buried in the tissues of the orbit, displacing slightly the inner third of the ciliary margin of the upper eyelid, but not interfering with the movements of the eyeball—hence no diplopia. In October, 1881, the tumor was opened by a surgeon, nothing escaping but an oily fluid. The tumor collapsed; the wound was dressed and healed rapidly. In about two months the cyst had regained its former size and remained so till the patient presented himself to me. The exploring needle proved the presence of fluid. Vision, both eyes = $\frac{20}{xx}$. No lesion in fundus oculi.

CASE IV.—On April 23, 1883, my friends Drs. Miller and Wirgman asked me to examine with them a case with the following history and conditions. H. G. B., aged twenty-six years, a patient of Dr. Miller. At the age of three years, a small tumor, about the size of a split-pea, was detected below the orbital ridge on the nasal side, buried in the tissues of the right upper eyelid. The tumor was of slow growth, for at the time of the operation, twenty-three years after its discovery, it had only attained the size of an almond. The tumor, upon digital examination, fluctuated; an exploring needle proved the presence of fluid; no lesion in fundus oculi, and the vision in both eyes was normal. The patient suffered no pain nor inconvenience. The tumor was removed by Dr. Wirgman to relieve the patient from the apprehension that a serious trouble might develop.

Dr. Miller recently informed me that the operation has been successful, as there is no evidence of the cyst returning.

The external appearances of the tumors were strikingly similar. The integument covering had a glistening, pink color, slightly translucent, moderately tense, and not adherent to the tumor. The sizes of the cysts vary; they are not pointed, but oval, in shape, having their greatest breadth immediately below the skin, while the small or pointed diameter lies buried deep in the orbit. Their point of development, apparently, is from the periosteum covering the nasal bone, finding their way to the surface, expanding where there is least constriction from the tissues, are of exceedingly slow growth, never interfering with the mobility of the eyeball nor movements of eyelid. These cysts are detected in early life, and, no doubt, like the dermoid, are developed in the embryonic period. The cyst-walls, which are thin, fibrinous, and elastic, cannot be easily dissected from the adjoining tissues.

The fluid contents of the sac are clear-yellowish, and of the consistency of honey. Microscopic examination revealed an oily fluid filled with fatty, granular masses, large oval cells, similar to mucous cells; no cholesteroline crystals, or dermoid debris.

Treatment.—The application of irritants externally, puncturing the cyst, injecting dilute tincture of iodine, signally fail to give permanent relief. An incision of not less than six lines was made with a sharp-pointed bistoury, or Beer's knife, into the sac, making the wound in the horizontal direction, and the

contents of the tumor emptied by gentle pressure. A point of nitrate of silver was then thoroughly applied to the inner walls of the cyst (great care must be observed to apply the caustic to the lower part of the cavity); by this procedure the inner walls are destroyed, thereby creating two inflamed surfaces which become agglutinated, and the cyst is obliterated. A small pellet of lint is usually kept in the external orifice of the wound for three or four days, to prevent closure, as the destroyed tissues will slough, and a slight discharge make its appearance. For the first six hours after the operation cold dressings are applied; after this, boroglyceride. There is no reaction, the cavity healing rapidly.

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MEDICAL PROGRESS.

CORROSIVE SUBLIMATE AND IODOFORM IN THE PUERPERAL STATE.—Valuable information is obtainable from the report of the discussion (in the Fifty-sixth Congress of German Physicians and Naturalists) on Kehr's paper on "Sublimate Injections" (*Archiv für Gynäkol.*, Bd. xxii., Heft 1). Mercuric chloride is shown to be odorless, and cheaper and more soluble than carbolic acid. Küstner, who obtained the uterine secretions by means of tubes direct from the interior of the womb, discovered that the micrococci, which normally abound in them, disappeared promptly after sublimate injections, but only slowly after carbolic injections. Bardeleben's experiments seem to show that a solution of 1 in 10,000 is sufficiently energetic. Ehrendorfer contributes an important paper, "Ueber die Verwendung der Iodoformstäbchen." He recommends iodoform pencils containing ninety to one hundred and fifty grains of pulverized iodoform, and fifteen grains each of gum-arabic, glycerine, and pure starch, which are easily soluble and flexible; they are thus readily introduced into the uterine cavity, after the genital tract has been irrigated with an antiseptic lotion. The iodoform was only used as an intrauterine pencil in labors which were abnormal, or which were followed by purulent endometritis, etc. He reports in detail twenty-nine cases, nearly all severe, in several of which the patient's condition was rendered desperate by the existence of gangrene, erysipelas, and other serious complications. König has shown that iodoform intoxication is rare when not more than ten grammes (about one hundred and fifty grains) of pure iodoform are applied.—*Medical Times and Gazette*, May 31, 1884.

ACTION OF PAPAYOTIN.—ROSSBACH, in the course of recent studies on this subject, finds that the juice of papaya, and papayotin and papain, can be given internally, by direct intravenous injection and by hypodermatic injection. He also finds that the drug may be used in cutaneous eruptions, for cancerous nodules of the skin, for the removal of diphtheritic membranes, to assist meat-digestion in the stomach and intestines, in dyspepsia and other diseases of the digestive organs, in cancer of the stomach, and for intestinal worms, which papayotin should kill.—*Deutsche med. Wochenschrift*, April 3, 1884.

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INTERMITTENT CHYLURIA.

AN interesting discussion, which took place at the Berlin Medical Society, on this still obscure subject, is published in the *Berliner klin. Wochenschr.* for March 10. PROF. SIGMUND reported an example which elicited the views of VIRCHOW, SENATOR, EWALD, and LITTEN.

The case was that of a German who, excepting a year spent in France during the recent war, had never been out of the fatherland. At the siege of Paris, he had dysentery for two weeks. At the beginning of 1883, he became weak and restless, sweated profusely, and complained of rheumatic pains in the back and thighs, and in the lower part of the abdomen and testicles. His urine was albuminous, and remained turbid after the albumen was separated. The turbidity was found to be due to molecular fat, of which there was 1.038 per cent. present. A power of 900 diameters revealed neither animal nor vegetable organism in the fresh urine. This fatty urine alternated with perfectly normal fluid, quite free from albumen and fat, the longest time during which the urine was free from chyle being seventy-two hours. It was present on an average once in every twenty-four hours, and appeared most frequently between 2.15 and 7.45 A.M. The condition gradually improved, but had not disappeared altogether.

As to the cause of the chyluria, Sigmund thought it could not be due to any disease of the kidneys, or that these organs could even play an intermediate rôle. He thought rather that a mechanical defect existed, as the result of which the two streams of urine and chyle commingled somewhere between the pelvis of the kidney and the beginning of the urethra.

This view is confirmed by a case reported by Havelburg, and is accepted also by Hoppe-Seyler. It explains the sudden occurrence, but not the intermission in the case reported, for this was found quite independent of the recumbent position, whether in natural sleep at night, or that during the day induced by chloral. While admitting that the cause of the disease had not yet been ascertained, he thought that chyluria, as it prevailed in the tropics and in temperate climes, are not one and the same thing.

Senator had recently met with a case of chyluria, also intermittent, in which a study of the blood by day and at night discovered no parasite, nor did the blood exhibit a fatty character, while it contained, in addition to albumen, hemialbumose, but no peptone, which had been found by Brieger and others, and no sugar. He thought, however, that in his case the phenomenon was influenced by rest and action.

Ewald thought the intermittent occurrence might be explained by the existence of a valvular fistula, which would be kept closed by a sufficient intralymphatic pressure, but which became patulous when such pressure was diminished.

Litten had observed intermittent chyluria in a case of intermittent fever, but had been unable to establish any relation between the chyluria and the pyrexia or apyrexia. The patient averred that he had had both conditions for a long time.

Virchow thought that the fact that the phenomenon appeared in the morning was in direct opposition to the explanation of Ewald and that it was difficult to follow the path the chyle must pursue in reaching the bladder. If the condition is one primary to the lymph vessels, the current must travel backwards, which is contrary to all experience—the absorbed chyle travelling toward the root of the mesentery and receptaculum chyli. On the other hand, the absence of sugar is the more remarkable, since in the analogous phenomenon—elephantiasis lymphorrhagica—sugar is present. One cannot but believe that in the latter condition the chyle passes through the blood to the periphery, and from the blood is passed into the lymph vessels; in a word, that a chylous state of the blood must precede the separation of the chyle. The case is different with chylous ascites. Here we must have an overdistention of the chyle vessels of the intestine and their rupture into the peritoneal sac. In the neighborhood of the pancreas, we also often have an ectasy of the lymph vessels, with colossal cyst formation. Prof. Virchow thought that further observations should be made, and especially that the blood should be examined at different times, with a view to determining the quantity of fat therein.

Senator considered that the experiments of Brieger, by which it was shown that the liberal ingestion of

fat by the patient produced no change in the urine, and especially none in its quantity; were opposed to the view of Ewald.

Ewald thought he had been misunderstood by both Virchow and Senator. He did not claim that the appearance of the chyle in the urine was the result of an increase of chyle in the chyle vessels after digestion, but rather to the opposite condition, such as occurs during fasting, whereby a fistulous communication between the lymph vessels and urinary passages is opened, which is closed by a distended state of these same vessels, just as in certain cases of pneumothorax the opening in the lungs is closed if the pressure within the chest is great, while it is open if the pressure diminishes.

It is scarcely likely that four men could have been found better qualified to explain the phenomena of chyluria, especially as it occurs in temperate climes, than those whose views have just been presented. Yet it is evident that nothing definite was added to our knowledge, although ingenious hypotheses were suggested.

It is almost certain that Sigmund is correct in his explanation as to one mode in which chyluria may occur; for we ourselves know of one instance of chylous hydrocele occurring in the practice of Dr. W. H. Mastin, of Mobile, Ala., in which the weeping lymphatic was actually exposed in a dissection,—appearing as a papular swelling, from the apex of which the lymph was observed to exude. Ligation of the papule was followed by a cure of the condition, whereas simple tapping previous to this operation had been ineffectual in preventing reaccumulation. The portion excised was sent to us for examination, and a cavernous lymphatic structure was easily demonstrated. That Ewald's suggestion would satisfactorily account for the intermittent character of the flow is also true, but difficult to prove, and until proof is adduced, it must be regarded as speculation, and not fact.

MYOCARDITIS.

J. WICKHAM LEGG's "Bradshawe Lecture" on "Cardiac Aneurism," for 1883, and W. H. WELCH's paper on the "Pathology of Myocarditis," read at the meeting of the American Medical Association, in May last, indicate a revival of interest in the subject of the non-valvular affections of the heart. These investigations, like others which, since the days of Corvisart, have been from time to time carried on in the same field, are wholly within the domain of pathology. When contrasted with the importance of this group of cardiac lesions, the amount of clinical work brought to their study is surprisingly small, and the results are still more insignificant. It is true that much attention has been bestowed both from the pathological and the clinical

standpoint upon fatty heart, the acute parenchymatous degenerations of fever and phosphorous poisoning, and the loss of tone in the heart-muscle which accompanies general debility. Without undervaluing the importance of these conditions, we do not hesitate to say that infrequency of occurrence, gravity of symptoms, and as an immediate cause of death, myocarditis, in its various forms, constitutes an affection not less worthy of attention.

Within recent years Rigal and Jubel-Renoy, in France, and L. M. Petrone, in Italy, have sought to clear up the symptomatology of myocarditis and to establish rules for its diagnosis. But the clinical papers upon this subject may be counted upon the fingers of one hand, and in a majority of the works on diagnosis, myocarditis does not find place even in the index. The causes of this are not far to seek. During the last fifty years, clinicians have been engaged in elaborating and refining the art of auscultation, and in analyzing its signs until the diagnosis of those diseases of the heart which are characterized by coarse alterations in structure is an almost perfect procedure. Meanwhile, the recognition of affections not manifest by specific signs, but by feebly marked deviations from the normal, and by a vague or blurred symptomatology, has been overlooked. At the bedside myocarditis is practically an unknown disease. Even when its presence is suspected, there are, as yet, no means by which a definite diagnosis can be reached. Examinations ending in uncertain results have little attraction for the practical physician. Hence, the unfortunate neglect into which this disease has fallen. We do not believe that the resources of clinical medicine are unequal to the task of recognizing most of the forms of this disorder, or, indeed, to that of tracing out the obscurities of its etiology. We believe it to be simply an untilled field that is rich in promise. The results of laboratory work will accumulate until sufficient data for clinical study are at hand. To arrange, differentiate, exclude, and classify, are processes that await only the accumulation of facts and cases. Already, much has been accomplished. It is now known that neither endocarditis nor pericarditis is a necessary accompaniment of myocarditis.

Welch has found myocarditis in the examination of cases which may be clinically grouped as follows: *First*, when there is no symptom of heart disease. *Second*, sudden death without previous heart symptoms. *Third*, sudden death preceded by one or more attacks of angina pectoris. *Fourth*, after cardiac insufficiency of a few days' standing. *Fifth*, in cases of old heart disease. Jubel-Renoy regards the diagnosis of chronic myocarditis as having attained already some clinical exactitude. He arranges the diagnostic data into three groups: 1. Enfeeblement of the systole, and of the pulse,

with augmentation of the frequency and persistence of regularity. 2. Moderate hypertrophy, varying within considerable limits, which are determined by the amount of dilatation present. 3. The common absence of murmurs; occasionally the occurrence of a soft, systolic mitral murmur, often temporary. Then *bruits de galop*, tricuspid blurring, pulmonary congestion, often unilateral, all coming and going until asystolism, gradually developing, brings the scene to an end.

ALBUMEN OR ALBUMIN.

ALBUMEN spelled with the vowel *e* in the last syllable has been used in the English language, probably for centuries, to indicate a nitrogenous principle which constitutes the chief part of the white of egg, and is found in the serum of the blood, chyle, synovia, serous fluids, etc., and sometimes also in the urine, generally as an abnormal constituent. It was evidently adopted directly from the Latin *albumen*, for white of egg. And the English adjectives, albuminous and albuminoid, have been properly spelled with an *i* in the third syllable, in accordance with the termination of the root, which is formed from the genitive *albuminis*. On the other hand, German writers, particularly physiological chemists and urinologists, have for some years spelled the noun with an *i*. Such is the spelling of Gorup-Besanez, of Hoppe-Seyler, Neubauer and Vogel, Hoffmann and Ultzmann, and others. But that this spelling is not the only one used by Germans is proved by consulting Lucas's Dictionary, German-English part, in which we find the word *albumen*, and *albumin* is not given.

Recently a few English-writing physiologists and urinologists, notably, Foster in his text-book of Physiology, have adopted the German orthography, which may also be said to be the French, although in that language the last syllable is *ine*. Dr. Millard, in his recent work on Bright's Disease, has also used this spelling, and in an article just published, expresses his surprise that English-writing physicians should continue to use the older style, but we think a careful examination of the history of the use of the word fails to justify his emphasis in the matter. The early German writers use the term *Eiweiss* so constantly for albumen that it appears to us not unlikely that albumin was a later introduction from the French, the terminal *e* of the last syllable being omitted. While we admit the desirability of uniformity, we do not see sufficient reason for English writers deviating from the original well-founded spelling of the word.

TUBERCLE BACILLI IN DEEP-SEATED TISSUES.

In some recent studies published in the *Wiener med. Wochenschr.*, March 29th, on tuberculosis,

WEICHELBAUM took pains to look for the bacillus tuberculosis in tubercles most deep seated, and distant from the external air, including the inner membranes of the brain, the thyroid gland, the endocardium, the liver, the spleen, the kidneys, the peritoneum, and the omentum, and in none of these situations did he fail to find them. The number, it is true, varied greatly, and sometimes sections of nodules from the liver and spleen contained but one to three, while in other tubercles, especially the pia mater and peritoneum, including the omentum, the number was so great that even with a low power the places of their accumulation could be easily recognized.

These observations are important, in comparison with those of Formad. Weichselbaum found that with the onset of caseation the number of bacilli diminished materially, and they were confined more and more to the periphery, or to the giant cells. According to Formad, on the other hand, bacilli are more numerous when the tubercle begins to soften.

Weichselbaum also ascribes the failure of many observers to recognize bacilli, to imperfect staining, of imperfect optical appliances, or to inexperience. He considers an Abbe's illuminator and an oil immersion system essential.

TRANSMISSION OF TUBERCULOSIS.

THE presence of bacilli of tuberculosis in the blood, in chronic tuberculosis, has a bearing upon hereditary transmission of tuberculosis, and also upon its spread by vaccination. For if a bacillus can reach the innermost tissues of the economy, why may it not travel also from the blood of the mother to that of the embryo? Why, too, may it not be transferred with the vaccine lymph. The possibility of either event dare not be denied, if Koch's views be acknowledged. At the same time, even admitting such possibility, it is plain that the chances of such an accident are so few, that we need not be influenced in our practice as to vaccination; while natural difficulties in the way of transmission from the blood of the mother to that of the embryo account for the frequency with which the children of tubercular parents escape the malady.

In like manner, it is a legitimate result of these facts, that it is not venereal disease alone which may be communicated from the male to the female in coitus. The man with tuberculosis of the seminal vesicles and urethra may inoculate the woman with whom he has intercourse, and thus produce a tuberculosis of the uterus and Fallopian tubes. Weichselbaum tells us of a case of tuberculosis of the seminal vesicles and urethra, in which the number of bacilli was enormous. A seminal fluid in such cases would naturally be loaded with bacilli, which, carried up

into the cavity of the uterus, might plant themselves in such favorable nidus as happens to be present. It is fortunate, in view of these facts, that tuberculosis of the genital apparatus of the male is as rare as it is, else we would find such mode of transmission more frequent. In the meantime it behooves practical physicians to bear these views in mind and apply to them the test of experience and observation.

SOCIETY PROCEEDINGS.

AMERICAN NEUROLOGICAL ASSOCIATION.

Tenth Annual Meeting, held in New York, June 18, 19, and 20, 1884.

(Specially reported for THE MEDICAL NEWS.)

WEDNESDAY, JUNE 18TH—FIRST DAY.

AFTERNOON SESSION.

THE VICE-PRESIDENT, DR. WILLIAM J. MORTON, read a letter of regret from the President, Dr. Robert T. Edes, of Boston, who was unable to be present, and then offered a few remarks himself, in the course of which he made a defence of specialism, particularly in the field of neurology. The specialists in this department, he claimed, had contributed more to general medical and surgical knowledge than any other class of writers, and this, he thought, justified the neurologist in his subdivision of scientific labor. Neurology is but following the general tendency of the times, and with better right than many other specialties, for it offers to its followers a field for investigations so vast, so fruitful, so impossible of exhaustion, that even if they should desire it they are precluded from equally extended investigations into other branches of medicine. To the sick the subdivision of labor is an advantage, since, other things being equal, this means skilled labor. Clearly, this field for work is all that the neurologist can attend to—it is certainly more than the general practitioner can keep up with. Dr. Martin also gave a retrospective glance over the past work of the Association, and referred briefly to its present condition.

He then introduced the PRESIDENT-ELECT, DR. ISAAC OTT, of Easton, Penna., who delivered the *Annual Address*.

The special subject to which this was principally devoted was a consideration of

THE PATHS OF THE VARIOUS FIBRES IN THE SPINAL CORD.

He said that it was now about seventy years since Charles Bell announced that the anterior roots of the spinal nerves were motor and the posterior sensory. Since then countless experiments had been made, and there had been a gradual increase in the knowledge of the physiology of the cord, which was due principally to improvements in technique. He said that he knew of no more deceptive thing than to assert the extent of disease of the spinal cord without a careful microscopic examination. In regard to the paths of coördination, there had been various opinions up to 1840, when Longet made numerous experiments upon the subject, and announced, as the conclusion, that the anterior columns

of the cord were motor and the posterior ones sensory. Prof. Schiff's idea of the spinal cord was that the posterior columns conducted tactile impressions, that the gray matter conducted in all directions afferent impulses which gave rise to affections of general sensibility and such afferent impulses as were concerned in reflex action. Dr. Ott then mentioned various views to show that the facts put forth were greatly in need of correction. After alluding to the experiments and remarks of Finch, Ludwig, Schiff, and others, he said that the observations of Ludwig proved that in the lumbar cord of the rabbit sensory and motor fibres ran in the lateral columns. Afterwards, with the assistance of Dr. R. Smith, he had himself proved the same statement to be true for the cervical segment of the spinal cord of the rabbit. To these experiments the objection was rightly urged that the animal was not permitted to live for a considerable time after the operation. But experiments of his own on kittens which sustained these injuries for several weeks proved that, in addition to the lateral columns, the anterior columns contained voluntary motor fibres. He then referred to a number of experiments on dogs by other observers, and remarked that only one recent authority still defended the theory that the motor and sensory fibres did not lie mainly in the lateral columns. All the later experiments went to show, he said, that the lateral columns are the principal conductors of sensation and motion, that the gray matter does not directly participate, that the anterior columns conduct voluntary motion. In regard to the posterior columns, he could substantiate Prof. Schiff's assertion that, after bleeding a rabbit and dividing the whole cord except the posterior columns, impressions of touch are conveyed by the posterior columns. In conclusion, he said that he should also like to state that decussation of fibres in the central nervous system is quite extensive, that the motor, sensory, vaso-motor, sudorific, and inhibitory (the two latter running in the lateral columns) are over each other.

AN EXHIBITION OF PREPARATIONS

was then made by DR. BURT G. WILDER, of Ithaca. The first one illustrated the *existence and circumscription of the portæ (foramina of Monro) in the adult human brain*, and he said that he was moved to present it, first, because he was not aware that any just such preparation had previously been made; and, secondly, because Mr. Duval had contended that the foramina of Monro did not exist at all in the adult human brain. He then described the manner in which the brain exhibited had been prepared. It was kept in the dura mater, and having been suspended by threads in a, at first, weak solution of alcohol, the infundibulum was removed, and, by means of a canula, alcohol at a temperature nearly of freezing was forced through the arteries for about ten days, by which process it was thoroughly hardened. The specimen plainly demonstrated to the eye the dorsal boundary of the foramen of Monro.

The second preparation exhibited was a portion of the brain of an infant at birth, hardened in the same manner, which showed distinctly the *presence of the crista fornicis*. This feature seemed to be always present in the brain of the cat, as he had examined the brains of hundreds of these animals, and had never as yet found it absent. In the human subject, however, it

disappeared as the child grew older; and, so far as he knew, it had never been observed in the adult. It was presumable that in all mammalian brains it existed at some period of life.

He had of late discovered two additional cases of *absence of the corpus callosum in the domestic cat*; but had, unfortunately, neglected to bring the specimens with him. The number of instances in which he had now found this wanting seemed to indicate that if brains were more frequently examined with proper care this anomaly would be found not to be so rare as had heretofore been generally supposed.

The last specimen which he exhibited showed the *covering of the cerebellum by the cerebrum in a young chimpanzee* whose brain was hardened within the skull. It was one which illustrated very well the necessity of making such preparations with great care. When the orbital lobe was made horizontal, the tip of the occipital lobe was seen to project about a millimetre beyond the cerebellum.

DR. AMIDON then exhibited a *human brain in which the corpus callosum was absent*, and said that he would read the history of the case at a future session.

DR. E. C. SPITZKA remarked that as one proceeded from the lower to the higher orders of animals the cerebrum and cerebellum both became more intricate, and that in the lower monkeys the overlap was much greater than in the chimpanzee and other anthropoid species.

A REPORT FROM THE COUNCIL

was presented, recommending the election of certain new members to the Association, but, as a quorum had not been present at this meeting of the Council, the report was, on motion, laid on the table.

The Association then proceeded to the nomination and

ELECTION OF OFFICERS

for next year, which resulted as follows:

President.—Dr. Burt G. Wilder, of Ithaca, N. Y.

Vice-President.—Dr. Leonard Weber, of New York.

Secretary and Treasurer.—Dr. G. M. Hammond, of New York.

Councillors.—Drs. W. R. Birdsall and W. J. Morton, of New York.

EVENING SESSION.

ELECTION OF NEW MEMBERS.

A report of the Council was read by the Secretary, recommending for membership in the Association Dr. G. Betton Massey, of Philadelphia, and Drs. George W. Jacoby and Sarah J. McNutt, of New York; and these candidates were then duly elected by ballot.

THE REPORT OF THE COMMITTEE ON THE HAMMOND PRIZE ESSAY OF THE AMERICAN NEUROLOGICAL ASSOCIATION

was then read. The subject proposed for the prize essay was, *The Functions of the Optic Thalamus in Man*. Only one essay had been received; but this was a very elaborate thesis, written in German. After giving a minute analysis of its contents, the report stated that it possessed great merit and constituted a valuable contribution to scientific literature, but that it hardly met the requirements demanded in a prize essay on the subject in question, since it treated the functions of the

thalamus in a too restricted way, while the part of it relating to the human physiology was scanty and inconclusive. The prize, therefore, was withheld for the present year.

In the discussion which followed the reading of the report of the committee, the opinion was quite generally expressed that the prize ought to have been awarded to the essay described, and the matter was, therefore, on motion of Dr. Wilder, referred back to the committee for further consideration.

DR. BURT G. WILDER, of Ithaca, N. Y., read a paper on

MACROSCOPIC ENCEPHALIC NOMENCLATURE.

His idea was to simplify names in use as much as possible and render them more flexible. There are two sets of terms recommended; the one applicable to toponomy and the other to encephalic organomy. Having alluded to one or two other modes of subdividing the brain, he mentioned that it could be divided into a series of five segments, the principal parts of which were the medulla, cerebellum with the pons, the optic thalami, and the cerebrum (the olfactory lobes being a division of the last). He proposed to take up the different parts of the brain in categories, and separate the heterogeneous mass of terms which was now so cumbersome. The technical names which he proposed were brief and composed each of a single word, and monomial technical nomenclature possessed the following advantages: (1) brevity, (2) flexibility, (3) adaptability, (4) capacity for uniform abbreviation. In speaking of the flexibility, he remarked that the names could readily be turned into adjectives. Thus, it was hardly admissible to make an adjective of *dura mater* or *pia mater*; but there was no difficulty whatever about the terms *dural* and *pial*. The adaptability of the nomenclature was shown by the fact that any single word might be adopted into any language based on the Roman alphabet. Professor Wilder then referred to a list which he had made out for the use of students and others. In it single words took the place of compound ones, as for instance, *dura* and *pia*, instead of *dura mater* and *pia mater*, *alba* for white matter, *calcar* for hippocampus minor, *hypocampa* for hippocampus major, *callosum* for corpus callosum, *cineria* for gray matter, *porta* for foramen of Monro, and *thalamus* for thalamus opticus. Then, there are certain abbreviations which are somewhat of the character of nicknames, such as *proen.* for *proencephalon* (cerebrum), *dien.* for *diencephalon*, *epon.* for *eponcephalon*, *mesen.* for *mesencephalon*, and *meten.* or *metencephalon* ("medulla" as far as the pons). Instead of the Latin word *ventriculus* (English, ventricle) he uses the Greek *κοιλια* (English, cele). The *ventriculi lateralis* thus became the *procelia* or *procele* (first and second ventricles, together with the *aula* and *portæ*); the third ventricle, the *diacele*; the fourth ventricle (its cephalic part), the *epicele*; the caudal portion of the fourth ventricle, the *metacele*; and the *aquæductus Sylvii*, or *iter a tertio ad quartum ventriculum*, the *mesocele*.

On motion of Dr. Amidon, the discussion of Dr. Wilder's paper was postponed until the following day.

DR. A. D. ROCKWELL, of New York, read a paper on

A CASE OF TONIC SPASM OF THE DIAPHRAGM (?).

The case had been under observation for a long time, and was believed to be unique in character. For this

reason and because its pathology was so obscure he presented it in the hope that light might be thrown upon it by some of the members of the Association. The patient was a lady, 35 years of age, and the mother of four children, who first began to suffer from the present trouble twelve years ago. The paroxysms which caused her so much distress were substantially the same both in kind and severity then as now, but their recurrence had been marked by uncertain and varying intervals. Before the present year they seldom occurred more than once a year, and sometimes two and three years intervened between the attacks. During the months of April and May past, however, the attacks had been very frequent. For some time they continued every day, and occasionally twice a day; so that her strength was fast becoming exhausted. Formerly no special hour marked the occurrence of the paroxysms; but latterly they had occurred in the morning between the hours of five and seven. They were described as follows: The patient is awakened out of a sound sleep by a vague feeling of pressure and distress, which soon develops into the symptoms now to be described. The pain, or rather distress (for there is nothing about it that even simulates anything of a neuralgic type), is felt about the lower end of the sternum and over the epigastrium, and extends to a corresponding point in the back. It is as if a crushing weight were being applied—as if the front and back were being pulled together. So sure is the patient in her own mind of these subjective phenomena that she has always declared that they could be caused by nothing else than a spasmodic contraction. The recumbent position is impossible; and so she moves about the room partially bent, or leans forward against any support, with short and gasping inspirations, pallid countenance, and with agony depicted in every feature, until the paroxysm subsides, or until the anguish becomes unendurable, and necessitates the administration of an anæsthetic. The pulse becomes weaker and somewhat less frequent, but is not at all irregular. The attack lasts from twenty minutes to three-quarters of an hour, and after passing away leaves the patient much prostrated. From this, however, she quickly rallies; but a bruised and sore feeling remains for many hours.

On one occasion, about two years ago, she awoke in the night with the paroxysm, which culminated so rapidly that before the chloroform, which was immediately resorted to, could take effect, she became quite pulseless and apparently lifeless; and it was only on pushing the anæsthetic that the pulse became stronger and the inspiration apparent. Several repeated administrations of the chloroform were necessary before the attack was finally broken up. For several years, as these paroxysms, were so unfrequent, and the general health continued good, chloroform was almost the only remedy employed, hypodermic injections of morphia having been found insufficient to cause relief. When they began to occur frequently, however, many efforts were made to prevent their recurrence. Electricity was tried in all its forms, together with a varied internal treatment; but without avail; and it was not until he resorted in despair to the use of the actual cautery that the attacks seemed to be at all controlled. To his great relief, none had occurred since its application, nearly a month before; and, what was also a great

source of relief to him, the general health and strength had markedly improved with the rest thus afforded.

That this was a case of spasm of the diaphragm the patient, who was not altogether devoid of anatomical knowledge, was firmly convinced. Having referred to other possible hypotheses in regard to the trouble, such as biliary calculus, disorder of the heart and malaria, and shown them to be untenable, Dr. Rockwell stated that, after years of observation in this particular case, he was himself quite convinced that the symptoms were due to a spasmodic contraction of the diaphragm, notwithstanding the absence of certain objective symptoms, such as expansion and immobility of the lower half of the chest, projection of the epigastrium and closure of the œsophagus, that he had found emphasized in print and speech. In connection with the apparent efficacy of the actual cautery, he referred to a paper which he read some years ago before one of the New York societies, on the use of the galvanocautery, one of the cases reported in which was kindred to and as unique as the one now related. The patient was suffering from a supposed organic stricture of the œsophagus. In consultation with the late Dr. Willard Parker, it was demonstrated that the obstruction to the passage of food was due to spasmodic contraction, excited by the introduction of anything into the tube. This condition had been persistent for several months, and the patient was slowly dying of inanition. Every spasm was associated with severe pain in the dorsal region of the spine. After the examination revealed the true character of the disorder, the actual cautery was applied along the spine over the seat of the pain, and resulted in immediate and permanent relief of the spasm, the patient thereafter being able to swallow solid food without the slightest difficulty.

DR. DANA inquired what reason Dr. Rockwell had for excluding angina pectoris, which was sometimes characterized by much the same symptoms as those described in this case; to which he replied that the seat of distress was so low down and so limited (the pain not being transmitted upward at all) that it had not occurred to him that the trouble might be angina. He had seen a number of cases of the latter affection, and the symptoms had been so different from those met with in this instance, that it did not seem possible to him that the disorder could be of that nature.

DR. BIRDSALL inquired whether he was entirely satisfied that two affections could be excluded, viz., hysteria and hepatic calculus, either of which might possibly offer an explanation of the phenomena noted; to which Dr. Rockwell replied that he was perfectly satisfied in his own mind that the trouble could not be hysteria, but that the matter of hepatic calculus he had not taken into consideration.

DR. DANA then said that it seemed to him a physical impossibility that the diaphragm should be in a state of tonic spasm without the abdominal walls being either tense or protuberant. One could not make any experiment with reference to the diaphragm, either in his own person or in others, without exciting such a condition of the latter. As well might we expect contraction of the biceps without the flexing of the forearm. The ability to swallow, especially solid food, would also seem to exclude spasm of the diaphragm.

DR. ROCKWELL remarked that he would like very much to know precisely what the trouble was in this case. Spasm of the diaphragm had seemed to him to offer the most satisfactory explanation of the symptoms; but that he was not by any means positive in this diagnosis was shown by the fact that he placed an interrogation point after the above designation in the title of the paper. As he had mentioned in his report of the case, there were certain points which seemed to militate strongly against the hypothesis of diaphragmatic spasm, according to the views generally accepted in regard to that rare affection; but it had occurred to him that there might perhaps be possible a slighter form of spasm of the diaphragm, in which the phenomena referred to were either absent or else developed to but a small extent. As regards the matter of deglutition, the patient had always been able to swallow a little whiskey during the paroxysms when the effort was made (which he thought would be possible even if the œsophagus was pressed upon in some degree), but she had never attempted to take any solid food.

DR. SPITZKA said he thought that the absence of protrusion of the abdominal walls during the paroxysms might perhaps be compensated for by a general expansion of the thorax; but Dr. Rockwell stated that he had never noticed that there was any such expansion. The only external phenomenon that he had observed was a slight fluttering appearance over the epigastrium. Dr. S. G. Webber having inquired where the cautery was applied, he stated that it was applied all along the spine, from the neck to the waist.

THURSDAY, JUNE 19TH—SECOND DAY.

AFTERNOON SESSION.

DR. WM. A. HAMMOND, of New York, explained that he thought the committee on

THE PRIZE ESSAY

were perfectly justified in excluding the thesis that had been presented to them, notwithstanding its very great merit, on the ground that it contained very little original research in regard to the functions of the optic thalamus in man (which was the point that was regarded essential in order to secure the prize).

On motion of DR. WILDER the minutes were amended by the striking out of so much of the motion which he had made at the previous session as referred the matter of the prize essay back to the committee for further consideration.

ELECTION OF MEMBERS.

The Council reported that they had unanimously recommend for associate membership, Dr. Danillo, of St. Petersburg, whose name was proposed by Dr. Wm. J. Morton. At a later session he was duly elected, and Dr. J. Leonard Corning, of New York, was elected an active member.

The discussion of Professor Wilder's paper on

ENCEPHALIC NOMENCLATURE

was then declared in order.

DR. WILDER begged leave to present a portion of his manuscript which, owing to the lateness of the hour, he had omitted to read at the preceding session. He said he desired to offer, as a basis for discussion, the three principal arguments which had been urged against the

adoption of the nomenclature which he proposed. The first of these was that some of the terms now in use were *indicative*. This, he acknowledged, was a strong point, and in certain instances it was unanswerable. At the same time, by using a single short word for a long compound name the difficulties of encephalic nomenclature would be rendered much less perplexing, as, for instance, the adoption of the simple word *iter* for the *iter a tertio ad quartum ventriculum*. In this way the term *biceps* had already been adopted into the vernacular. All the names of the new nomenclature could be learned in a week's time, and if their acquisition were attained gradually by the student, no effort would be required in the matter. Such a short and simple word as *lenum*, he certainly thought, was preferable to a long and high-sounding term like *torcular Herophili*. The second argument was, that some of the old designations were commemorative, as the *foramen of Monro* and the *fissure of Sylvius*; but as many of them were altogether unjust, he did not think the objection had much weight. The third was, that most of the present terms were familiar; but this seemed to him a puerile argument, which applied with equal force to almost every advance in the scientific world. The proposed nomenclature might necessitate a little effort on the part of those already familiar with anatomical terms; but the beginner would be able to learn the new names much more quickly than the old. In conclusion, Dr. Wilder read the following resolutions, which, he said, would in due time be presented for the consideration of the Association:

Resolved, That in the opinion of this Association the advancement and dissemination of an accurate knowledge of the macroscopic anatomy of the brain will be facilitated by substituting for many of the polynomial terms, technical and vernacular, now in use, technical names which are brief and consist each of a single word.

Resolved, That we recognize the advantages of using such monomial compound terms as antiplexus, diatela, mesocœlia, hemiseptum, præcommissura, medipedunculus, postcornu, and cognate words proposed by Professor B. G. Wilder, and see no serious objection thereto.

Resolved, That there be appointed by the Chair a committee of five on macroscopic encephalic nomenclature, with instructions to report at the next meeting a list of such terms as, in their judgment, may properly be recommended for use.

DR. DANA said that he had taken a great deal of interest in Professor Wilder's work of reform, and the matter had appealed to him all the more forcibly as a teacher himself. He begged leave, however, to offer a few criticisms that had occurred to him. Many of the points advocated by Dr. Wilder, he thought, were admirable, and he had no doubt that they would eventually be adopted. But, on the whole, it seemed to him that Dr. Wilder took too much the position of an anatomist and of an anatomical teacher. It was desirable to have terms that medical students could acquire readily; and this, he thought, was not the case with many of the proposed names. Dr. Wilder seemed inclined to sacrifice too much for the sake of brevity, and the adoption of such abbreviations as *dien*, for diencephalon, and *meson*, for mesencephalon, he considered entirely unjustifiable and

unnecessary. There was no harm in giving the full word, which conveyed some meaning to the student, and therefore assisted him in the acquisition of the technical terms. As well might we adopt a series of arbitrary words from the Chinese or Japanese as such "nicknames" as these. Dr. Wilder also showed a strong tendency to increase the already large number of anatomical landmarks in the brain, and thus render the matter more complicated, instead of simplifying it.

Thus, he thought, there was no necessity for the introduction of the term *ripha* (defined by Dr. Wilder as the line formed by the rupture of the endyma along the lines of its reflection from the entocœlion surfaces) to describe a little artificial part of the brain. He thought it would be much better, therefore, if he would devote his energies to reducing the number of terms already in use, instead of manufacturing new ones. He believed, too, that he made a mistake in endeavoring to substitute new words for some of the old ones which were universally received and understood, and which there was no necessity whatever for changing; as, for instance, *alba*, for white matter, and *mesocèle*, for the aqueduct of Sylvius. His idea seemed, simply, to get a monosyllable instead of a polysyllable in many instances. He objected also to the substitution of *κοιλία*, or *callia*, for ventricle, and to the formation of adverbs by the termination *ad*. Finally, he considered it a bold thing for one man to attempt to revolutionize an entire nomenclature; and thought that if Professor Wilder was really desirous of having his views adopted, he ought to submit them to an international commission, rather than to try to secure this end single-handed. At the same time he wished fully to recognize the great value of the services which he had already rendered in the direction of facilitating the study of the brain.

DR. BIRDSALL thought that the Association ought to be proud to subscribe to the admirable system devised by Professor Wilder, whose researches had reflected so much honor upon it. The only valid objections that could be urged against it, he believed, were in reference to certain details of terms, and he himself strongly objected to the nicknaming of scientific words. If the word meant anything it should be retained as a whole, provided it was not too cumbersome. In writing, it was often desirable to make use of abbreviations, but there was no necessity for employing them in speaking. Dr. Dana's objection that Prof. Wilder wished to multiply rather than reduce the names, he thought was not a sound one; for while there were many little points about the brain which had already received names, but were of no practical significance whatever, so far as known, there were others which were of essential use, especially to the comparative anatomist, which had not as yet been designated by special terms. As to whether the object desired by Prof. Wilder could not be secured by himself or by the action of societies was, he thought, an open question; especially when it was remembered what had been accomplished by such men as Orne and Huxley. Single individuals might make up consistent nomenclatures, and he thought Prof. Wilder showed a very generous spirit in expressing his willingness to submit the results of his labors to the judgment of a committee, as he had proposed to do. He had one suggestion to make, and that was, that if the committee arrived at any conclusion in regard to this matter, it

should invite the coöperation of learned bodies in Europe, so that, if any changes were thought desirable, their adoption might be the more readily secured throughout the scientific world. This kind of work was more of an international character.

DR. WILDER remarked that in his resolutions he had carefully excluded all reference to those abbreviated terms which seemed to excite the most opposition. In regard to his nomenclature in general, he would say, that if those present were as familiar with some of the other anatomists as they were with Quain and Gray, they would find that he had in reality originated very few of the terms employed.

On motion of Dr. Wm. A. Hammond, the consideration of the resolutions read by Dr. Wilder was postponed until the session of the following day.

DR. G. M. HAMMOND, of New York, then read a paper on the subject,

CAN LOCOMOTOR ATAXIA BE CURED?

He said that, as time had passed, reports of cases of this affection cured had come before the profession, many of them made by men of such undoubted learning and high reputation that we could not afford to overlook them. The question, therefore, that arose was, Are these true cases of ataxia, or is there some other morbid condition of the spinal cord which, when present, gives rise to symptoms similar in every respect to those resulting from sclerosis of the posterior columns?

He had collected a few cases of undoubted cure of what was supposed to be locomotor ataxia, and he thought that their histories and the different methods of treatment employed went far to settle the question of the curability of posterior spinal sclerosis.

Case I.—The patient, whom, he said, he was fortunately able to present before the Association for the second time, was the subject of a short paper which he read last year. He then exhibited him as a case of locomotor ataxia cured, and he was happy to say that the cure had so far stood the test of time. Having related the history in detail, Dr. Hammond stated that when the patient came under his care he applied the actual cautery several times to the spine, together with dry cups, the ether spray, and static and galvanic electricity. From this time the patient began to improve until complete recovery, with return of the tendon reflex, ensued. On this latter point he had him tested by several members of the Association at the meeting in June last, and he had had no treatment whatever since then.

Case II.—Reported by Desplot (of Lille) in *L'Union Médicale*, Nov. 29, 1883. Patient 54 years of age. Had contracted syphilis twenty years previously. Symptoms of locomotor ataxia of five years' standing. Treated with Gibert's syrup and mercurial frictions. In five days an amelioration of the symptoms, and in five months a complete cure.

Case III.—Under the care of Dr. Wm. A. Hammond. Two years ago presented all the symptoms of locomotor ataxia. A specific history admitted. Treated with iodide of potassium and bichloride of mercury, alternating occasionally with nitrate of silver. The actual cautery was also applied to the spine, as well as dry cups, the ether spray, and galvanism. A complete cure gradually resulted, though there had been no return of the tendon reflex.

Case IV.—Reported by Schultz, of Heidelberg, in 1881. Patient, who was 48 years old, first seen in April, 1871. Had had symptoms of locomotor ataxia for a year, and had constantly been growing worse. Marked improvement followed the use of the galvanic current, and in two years he was considered completely cured. Several years afterwards he died suddenly during an attack of acute intoxication, up to the time of the latter having continued perfectly well (with completely restored tendon reflex on both sides), with the exception of slight enuresis both at night and through the day. At the autopsy, however, diffuse degeneration of the posterior columns in the lumbar region was found, together with degeneration of the outer division of the posterior columns of the dorsal region and slight degeneration in the column of Goll. In such a case, Dr. Hammond thought, we were forced to conclude that some other condition of the cord, coexisting with or occurring separately from the sclerosis, was capable of producing a majority, if not all, of the symptoms of ataxia; and that this morbid state, under certain conditions, was capable of being removed by appropriate treatment. In support of this view, he quoted the following cases:

Case V.—Characteristic case of ataxia reported by Desnos. The patient died, and the autopsy showed that there was intense congestion of the cord, but not the slightest evidence of sclerosis of the posterior columns, or, in fact, any part of the cord, could be discovered.

Case VI.—Also reported by Desnos. Patient 24 years old. Specific history. Prompt recovery under anti-syphilitic treatment. Desnos regarded this, also, as a case of congestion.

Case VII.—Reported by Dr. Henry Lyman, in the *Chicago Medical Journal and Examiner* for January, 1883. Well-marked symptoms of ataxia. At first treated with ergot, nitrate of silver, and galvanism, but did not improve. Diagnosis of ataxia confirmed by Drs. Seguin and Beard, of New York. Then, after using iodide of potassium and bichloride of mercury for some time, in addition to the nitrate of silver, some of his friends advised him to try Junod's boot. This he used on both legs, besides dry cupping on both sides of the spine, and in two years from the time he was first seen he was found to have completely recovered, with the exception of some slight degree of ataxia in the movements of the feet. The tendon reflex, however, had not returned.

Having briefly called attention to a few of the most interesting points in connection with the more striking of the above cases, he said that from a study of them he had arrived at the following conclusions:

1st. That the absence of the patellar tendon reflex in locomotor ataxia is not always caused by sclerosis of the posterior columns.

2d. That sclerosis of the posterior columns may exist without being accompanied by the ordinary prominent symptoms of ataxia.

3d. That congestion of the posterior half of the spinal cord may give rise to most, if not all, of the symptoms of locomotor ataxia.

4th. That it is impossible during life to make a differential diagnosis between posterior spinal sclerosis and posterior spinal congestion.

5th. That posterior spinal congestion is curable.

6th. That there is no evidence to show that sclerosis once existing in the spinal cord has ever been removed.

7th. That those cases of so-called locomotor ataxia which have been cured are simply cases of spinal congestion, more profound in the posterior half of the cord.

DR. WILLIAM A. HAMMOND thought that the author of the paper was probably correct in his opinion in regard to congestion of the cord, and said that a case had lately come under his care which went to confirm the truth of this hypothesis. Just about a month ago he was consulted by an English weaver, residing in New-ark, and a man of great intelligence. At that time he was suffering from all the symptoms of well-marked locomotor ataxia, the lancinating pains in the limbs being of excruciating severity. There was, apparently, a history of syphilis, but the man was not at all sure whether he had had syphilis or not. He was ordered the iodide of potassium in large doses; but there had been no improvement whatever at the end of two weeks, although he was at that time taking sixty grains of the iodide three times a day, in addition to one-thirtieth of a grain of the bichloride of mercury. He then gave him codeia (which Dr. Hammond had found to be the best remedy for the relief of the lancinating pains of locomotor ataxia), to insure sleep at night, and one-quarter of a grain of nitrate of silver three times a day. The improvement that followed was extraordinary, and two weeks afterward all the symptoms of ataxia had entirely disappeared. The tendon reflex, which had been lost, was not only restored, but was found to be exaggerated. He did not know how else to explain this case except on the hypothesis of congestion of the posterior columns extending to the lateral columns of the cord.

DR. ROBERTS BARTHOLOW agreed with the author that some cases of so-called locomotor ataxia could be cured; but, what kind of cases, said he, were they? Some years ago he had cured a case with the iodide of potassium, but it was caused by mercury; and he had no doubt that there were to be met with at present many cases of metallic poisoning the symptoms of which were perfectly typical of true ataxia. Having spoken of the cases due to syphilitic gummata, which, he said, could be cured by iodide of potassium, he referred to those due to chronic syphilis, in which there was a true sclerosis, in consequence of the constitutional effect of the disease, and said that a sharp distinction ought to be made between these two classes of cases. Furthermore, he wished to express his conviction that under the use of remedies which were now available, and to one of which he would presently call the attention of the Association, many cases of genuine locomotor ataxia could be arrested. It was true that lost parts and the *status in quo* could not be restored, but it was possible, at all events, to prevent the degenerative tissue-changes from progressing any further.

DR. J. LEONARD CORNING thought that the cases referred to by Dr. Hammond in his paper were convincing. The fact that the autopsy in a well-marked case of locomotor ataxia revealed nothing but congestion seemed to him very conclusive sort of evidence; evidence that could not but be accepted. The effects of fluctuations in the blood-supply inside the cranial vault were recognized, and he did not see why they

should not be also in regard to the cord, although the vascularity of the parts might not be so great. One practical point of importance was, he thought, that—granting that a hyperæmia was the starting-point of the affection—we could, by relieving this condition, arrest its further progress.

DR. G. M. HAMMOND stated that, being obliged to withdraw before the conclusion of the discussion, he wished to say a few words on the remarks that had already been made. He did not consider, he said, that the cases with symptoms resembling those of locomotor ataxia which depended on spinal congestion and metallic poisoning, were cases of true ataxia. The existence of sclerosis was essential to constitute this disease, and sclerosis, he was convinced, was never cured. As illustrating this point, he referred again to Schultze's case, which had been described in his paper.

DR. S. G. WEBBER said that Dr. Hammond had reported his case as the first in which the tendon reflex had been known to return. Last year, however, he had himself reported two or three cases which had had all the symptoms of locomotor ataxia, and yet in which the tendon reflex was restored. One of these cases he did not suppose to be a genuine one of ataxia. The others were more evidently cases of ataxia, and one of them had entirely recovered. The patient had since died of a cardiac complication, but, although there was an autopsy, the spinal cord had not as yet been examined. Déjerine had found at the autopsy of a case supposed to be one of locomotor ataxia that the pathological condition was really multiple neuritis, and it therefore seemed probable that some of the cases in which cures were reported were of this nature, instead of being cases of ataxia. He should question, he said, whether hyperæmia of the columns of the cord could continue for weeks and months without giving rise to organic changes; and such a condition was, at all events, not yet proved.

DR. A. D. ROCKWELL remarked that, after all, the cases of locomotor ataxia, or supposed cases of the disease, in which a cure was reported were so few in comparison with the whole number of cases that they were really of very little significance. In looking over his notes, he found that he had treated forty-four cases of the affection. In the beginning, he thought that he had cured a number of cases, but he was now convinced that they were not genuine instances of ataxia. He also had read of one case of peripheral multiple neuritis which had been mistaken for this disease. In cases simulating locomotor ataxia, he thought that static electricity was of considerable benefit, and both Dr. Morton and he had reported cases cured by means of this agent.

DR. MASSEY said that he had seen one case cured. This was a gentleman who was a mountain climber, and who took no care whatever of his health. In 1866 he had syphilis. When he came under observation he had nearly all the symptoms of locomotor ataxia in its earlier stages. In addition, he suffered from profound melancholy. For one year he continued to grow worse, in spite of treatment, but he then began to improve, and when seen by Dr. Massey three years after this time, he was apparently perfectly well, though the tendon reflex had not been restored. For the first two weeks of his treatment he was placed on large doses of iodide of potassium, but this seemed to have a bad

effect, instead of causing improvement. Afterwards the treatment consisted of the use of protoiodide of mercury, galvanism, and the application of the faradic brush to the spine.

DR. C. K. MILLS said that he had seen a number of cases of pseudo locomotor ataxia cured, but he had never yet seen a case of the regular type get well. By this he said he meant that type which was recognized pathologically as posterior spinal sclerosis. The solution of the cure of pseudo cases had already been referred to in the discussion, and among the kinds of cases which were susceptible of cure were those due to syphilitic gummata of the cord, to meningeal inflammation (sometimes involving also the periphery of the cord itself), and multiple neuritis. In regard to the latter, he stated his belief that this affection sometimes eventually resulted in locomotor ataxia. He also referred to neuralgic and hysterical cases, and said that a knowledge of having been exposed to the danger of syphilis sometimes gave rise to the latter. In such cases the tendon reflex was not absent, as a rule. Congestion of the spinal cord, he thought, might give rise to many of the symptoms of posterior spinal sclerosis; but it was his own impression, that one could rarely say that there was a localized congestion of certain parts of the cord, as the posterior columns, for instance. It seemed more probable to him that there was a condition of meningeal congestion and irritation, or possibly of actual inflammation of the meninges. The point upon which he wished to lay special stress was, that, after all, the regular typical form of locomotor ataxia, recognized as dependent on extensive sclerosis of the posterior region of the cord, was not curable.

DR. MASSEY remarked that the case which he had described was evidently a typical one in all its symptoms (including diminution of sexual power), except that the pains are not so severe as was frequently the case. The patient was excessively nervous, but a man not at all given to introspection, and he thought that there was no hysterical element in the case.

DR. BANNISTER, of Kankakee, Illinois, mentioned two cases, in which most of the characteristic symptoms of locomotor ataxia were present, but which he believed were in reality instances of myelitis. There were no autopsies, however, to confirm this opinion.

DR. BIRDSALL said that he had had one case which he might have reported cured if he had done so in time; but after about a year's interval new symptoms began to develop. The patient was a woman, who experienced great relief for eight or nine months under the use of galvanism, etc. Up to that time the characteristic Argyll-Robertson pupil had not developed; but within the last six months this had appeared, and, in addition, there was now commencing atrophy of the optic nerve. There had been an increase of pain, but now in a new area, the upper extremities. There had been no return, however, of the former ataxic symptoms, and the tendon reflex had been restored. Here, then, was a case which at the end of a year, and for six months afterward, could have been reported cured; and, as far as the ataxia was concerned, this was still the case. This seemed to be an irregular case, in which other parts besides the posterior columns (in all probability the lateral columns) were affected. Dr. Birdsall also mentioned another case which was cured, as far as

ataxia was concerned, although the patient's patellar tendon reflex was still abolished. It seemed to him that many of these cases could show a long interval of time before the upper part of the end became invaded, and during this interval of repose were liable to be reported as cured. On the other hand, he believed there were many functional cases, and that there were still others that were due to localized lesions involving the lateral columns. In regard to the matter of congestion, he thought this was a speculative point which could not be definitely pronounced upon at present; but he agreed with Dr. Webber, that it was difficult to accept the proposition that congestion could continue for an indefinite period without giving rise to organic changes.

DR. S. G. WEBBER, of Boston, then read a paper on

MULTIPLE NEURITIS.

Neuritis, he said, had lately assumed an importance which had not formerly been accorded to it, and it had only been within a very few years that a more general inflammation of the nerves had been recognized. Having given a *résumé* of the bibliography of the subject, he said that up to this time he had found records of six cases without autopsies and twelve with autopsies. He then proceeded to relate in minute detail, four cases which had been under his own care at the Boston City Hospital. One of these had been in a boy and the other three in women. One of the latter was almost moribund when admitted, and died the next day afterward; and another died seventeen days after admission, there being in this case various complicating affections. It was from this last patient that the specimens exhibited were taken, and numerous sections of the spinal cord, as well as of the nerves all over the body (nearly all of which were affected), were made. These cases, he thought, were sufficient to serve as examples. During the past autumn and winter there had been eighteen undoubted cases of multiple neuritis under treatment at the Boston City Hospital, and fourteen of them had been under his own observation. In addition, he had seen one case in private practice, and one other case in a child, in which the diagnosis was somewhat doubtful between neuritis and infantile paralysis. Of the nineteen undoubted cases, the youngest was nine years old, and the oldest fifty-one. More than half were between twenty and thirty years of age. An interval of from one week to four months elapsed in the different cases, between the beginning of the attack and admission to the hospital. The stay in the hospital varied from two days to six months, and, as a rule, the patients were not well at the end of four months after the trouble began. In five cases, taking cold was attributed as the cause of illness; but in the others there did not seem to be any assignable cause. Nine of the patients were females and ten males, and three deaths occurred among them.

Disturbance of sensation was one of the most constant and prominent symptoms, and the pain was described variously, as aching, burning, shooting, and boring. There was, as a rule, much hyperæsthesia of the skin, general tenderness over the muscles, and very marked tenderness along the tracts of nerve-trunks, which could often be readily traced by the sensitiveness of the parts. There was, at first, stiffness, and afterwards paralysis. The limbs became more or less contracted (being for the most part in a state of semi-

flexion); and in some instances the contraction was extreme, the heels, for instance, being drawn up to the back of the thighs, and the knees nearly to the chest. Having dwelt for some time upon the condition of the various reflexes, which were not uniform (though the tendon reflexes were generally absent), he mentioned that the temperature and pulse were usually raised in the early stages, and in the two fatal cases this continued throughout the period when they were under observation. There was sometimes œdema of the limbs, and even of the face, and if there was also excessive smarting, as occurred in a number of instances, the disease was liable to be mistaken for rheumatic fever. The neuritis began most frequently in the legs. In fatal cases the disease made rapid progress.

Now that the peculiar symptoms of this affection had been clearly pointed out, the diagnosis was not difficult, except, perhaps, in slight cases. The diseases which it was most likely to be confounded with were, anterior poliomyelitis, spinal meningitis, locomotor ataxia, progressive muscular atrophy, lead paralysis, and acute rheumatism. Three or four of the cases mentioned, were supposed to be of rheumatic character when first admitted to the hospital.

In twenty patients suffering from the disease known in Japan as kak-ke, Scheube, who believed it to be due to a specific poison, had found neuritis in every case; and recently Dr. J. B. Lacerda had claimed to have discovered the bacillus of beri-beri. The nature of the pathological process in these cases was generally believed to be an inflammation of the nerves; but in September last, Erb had suggested (1) that it was possible and probable that purely functional disturbances of the trophic centres in the spinal cord produced anatomical disturbances (degenerative atrophy) in the periphery (the motor nerves and muscles); and (2) that it was possible that besides the complete destruction and separation of the trophic centres, yet other pathological processes occurred in them, which need not always go so far as to show themselves in the form of total degenerative atrophy of the peripheral regions of these centres. It had been shown, however, that Erb's hypothesis was less likely to be correct than that which referred the lesion to a peripheral neuritis. Having described the degenerative changes in the nerves as found in the case from which the sections were exhibited, and referred to the observations of Gombault, in rabbits, in which he had produced neuritis artificially, Dr. Webber said that, considering the changes found about the annular constrictions, it was reasonable to consider the disease as neuritis, not dependent upon changes in the spinal cord.

In most cases the patients recovered more or less completely, but recovery was very slow in severe cases; much time and patience being required to straighten the contracted limbs, and strength being regained very gradually. He was not sure, he said, that any treatment shortened the attack. Salicylic acid seemed to cut short the pain in some cases, and when it returned it was not quite so severe. It was necessary to give morphia to relieve pain. A four to five per cent. solution of carbolic acid applied on cloths to the painful limbs was of decided advantage. Ice to the spine felt grateful in one case, and hot water in another, while blisters over nerves seemed to be of advantage in one

or two instances, especially in a case in which the hyperæsthesia was very persistent. He thought it well to try to avoid or diminish the extent of the contraction by gentle extension or position of the limbs, a sandbag to rest the feet against, etc., as the contractions were difficult to overcome. To do so, required a persistent use of massage, extension, and electricity.

DR. ROCKWELL said that in a case of his in which he made the diagnosis of multiple neuritis, what annoyed the patient most was burning sensations in the limbs. After trying various agents in vain for the relief of this, he finally stripped the patient naked, and touched the sensitive points all over the body with the actual cautery. He repeated this half a dozen times during the next six weeks, and it was followed by great relief. Whether this procedure had any ultimate effect on the course of the disease he could not say, as the case passed out of his notice.

DR. BIRDSALL thought that Dr. Webber's contribution to this subject was one of great importance, as multiple neuritis was just beginning to be studied. He was not certain but what Erb was correct in his hypothesis that the affection was really dependent on cerebral changes; but this was a theory which no one could prove at the present time. It was certainly very interesting that such a group of cases as Dr. Webber had had the opportunity of studying should have occurred so nearly at the same time.

DR. R. W. AMIDON presented the brain taken from a patient suffering from *Word-deafness and Blindness, without Paralysis*, but omitted the reading of the case in order to permit DR. ROBERTS BARTHOLOW, of Philadelphia, to read a paper on

SOME USES OF THE CHLORIDE OF GOLD AND SODIUM IN NERVOUS AFFECTIONS.

Having stated that gold was mentioned in the *Anatomy of Melancholy* as a cure for that affection, and quoted the passage of the *Canterbury Tales* in which Chaucer says that "Gold in phisike is a cordial," he spoke of its use by the Arabian physicians in certain mental and nervous diseases; after which he referred to Trousseau's and Hahnemann's researches in regard to it. The homœopaths used it, he said, for the relief of hypochondriasis and of increased discharge of urine. Its principal effect was probably that of an alterant, or—as such agents are now more properly called—a promoter of tissue metamorphosis. It was of threefold service, he thought: *first*, as a general alterant; *second*, in nervous diseases; and, *third*, in genito-urinary diseases. The preparation which he himself preferred was the double chloride of gold and sodium, as this was readily diffusible, while the simple chloride of gold was diffusible to a very small extent. He used it in doses of one-twentieth of a grain, two or three times a day. It was a good tonic, and increased the number of red globules in the blood; but its chief effect was upon connective tissue. Hence, in pathological conditions in which the connective tissue was involved, it was likely to prove of service. In sclerosis of the posterior columns of the spinal cord, he did not claim that lost parts could be restored, but his experience with this remedy had been such as to convince him that by means of it the disease could often be arrested, and the further progress of the pathological changes permanently

stopped. He advised that its use in locomotor ataxia should be supplemented by the application of the faradic brush to the spine. He had also used the chloride with remarkable success in fibroid kidney. In the form of hypochondriasis connected with changes in the cerebral vessels it was very effective. He recommended it in various forms of spasm, among which he mentioned asthma and laryngismus stridulus, and said that in the latter affection one of his medical friends now used no other remedy. In certain cases of sexual debility accompanied by extreme hypochondriasis no remedy was so serviceable, and it seemed to him that there was some truth in the ancient motto that "Gold is a cordial to the mind." In simple sexual debility its use was sometimes followed by remarkable increase of power, and in dysmenorrhœa and in chronic metritis with scanty menstruation it had been found to be of benefit. More extended experience was, of course, needed to prove what the real position of this agent was in medicine.

[[DR. DANA said that he had a female patient suffering from hysterical melancholia, who had previously been under the care of a homœopath, who said that the latter had given her gold (in first dilution), and that it had always given her relief. Whether this was a mental or a therapeutic result he was not prepared to say.

DR. BANNISTER said that he had once treated some patients suffering from melancholia with chloride of gold for a number of weeks, but had not observed any effect from its use. He had, however, never used the double chloride of gold and sodium.

FRIDAY, JUNE 20TH—THIRD DAY.

AFTERNOON SESSION.

COMMITTEE ON NOMENCLATURE.

On motion of the Secretary, Dr. Wilder's resolution providing for the appointment of a committee of five on macroscopic encephalic nomenclature, with instructions to report at the next meeting a list of such terms as in their judgment may properly be recommended for use, was adopted, and the Chair appointed the committee as follows: Dr. E. C. Spitzka, Chairman, and Drs. B. G. Wilder, T. A. McBride, W. A. Birdsall, and L. C. Gray.

On motion of DR. BARTHOLOW, the other two resolutions offered by Dr. Wilder were referred to this committee.

The Vice-President, Dr. Birdsall, having taken the chair, THE PRESIDENT, DR. ISAAC OTT, read a paper on

THE RELATIONS BETWEEN INJURIES OF THE SPINAL CORD AND THE EXCRETION OF CARBONIC ANHYDRIDE.

By means of Arsonval's calorimeter and Voit's respiration-apparatus, he has made a series of experiments upon the lower animals (rabbits and cats), and has arrived at the conclusion that injuries of the cord produce, as a rule, an increase of the excretion of carbonic anhydride. It does not seem to make any difference whether the gray or white matter, or both together, are injured, the increase ensues just the same. Although the calorimeter was kept at 100° F., in only two cases out of seven did he see any increase of temperature. In all the others it fell below normal.

DR. G. L. WALTON, of Boston, read a paper entitled
A CONTRIBUTION TO THE STUDY OF HYSTERIA BEARING
ON THE QUESTION OF OÖPHORECTOMY.

The question of oöphorectomy, he said, should be limited to those cases in which the hysteria is secondary to abdominal trouble, and carefully exclude those in which the menstrual irregularities and ovarian tenderness are secondary to the hysteria. Whether the operation should be limited to cases of organic pelvic disease cannot be determined with certainty; but this is at present the safer plan. The most plausible explanation of hysterical hemianæsthesia (common after ovarian irritation) is that of spasmodic contraction of the cortical bloodvessels of the brain, suspending the function of the cortical nerve-cells through lack of nutritive fluid. This theory explains best, for example, the rapid onset and disappearance of the symptoms; for we know that a thought is sufficient stimulus and an instant sufficient time to cause the most extreme changes in the calibre of bloodvessels, as instanced in the blush, on the one hand, and the pallor of fear on the other. That such sudden changes take place in the brain, as well as in the integument, is shown by the phenomenon of fainting, in which the cerebral functions are entirely suspended, presumably through the lack of blood. The transfer of anæsthesia from side to side with succeeding oscillations is suggestive of the ebb and flow of a fluid. That vaso-motor irregularities are common in hysterical patients in other parts of the body as well as the brain, is evidenced by the frequency of chills, flushes, and ischæmia, sudden swelling of the joints, etc. Weight was thrown on this theory, he continued, by the case published in a paper by him on oöphorectomy in hysteria in the *Boston Medical and Surgical Journal*, of June 5, 1884. In this case, right-sided hemianæsthesia coincided with left-sided angio-spasmodic migraine and left-sided intermittent retinal ischæmia (the bloodvessels of the right eye being normal). The only explanation to be offered was that spasm of bloodvessels by implicating the membranes caused the pain by extending into the cortex, the hemianæsthesia of the opposite side, and by reaching to the eye evidenced itself in the abnormal contractility of the retinal vessels of the same side. The starting-point in this case was pelvic disease, probably by irritation of the sympathetic nerves supplying the ovarian region. In favoring this theory he said he was dealing with a tangible phenomenon of which we had constant examples, and could not, therefore, be accused of a flight of imagination. He had failed to produce "transfer" in hysteria secondary to pelvic disturbance, but had observed it frequently in cases of constitutional hysteria. This was probably because the unilateral spasm was kept up in the one case by the organic trouble, while in the others the sympathetic irritability is alike on both sides of the brain; so that while the vessels are contracting on one side, they are dilating on the other. In conclusion, the case referred to (the principal points of interest in regard to which were given in the paper mentioned) was reported in detail. The patient was operated on by Dr. Barss; the removal of both ovaries, which were found to be in a state of cystic degeneration, being followed by marked improvement in hysterical convulsions and hemianæsthesia of thirteen years' standing.

DR. E. C. SPITZKA said that he had given some serious attention to this subject, and that he was free to confess that he was, at the present time, very far from being as clear in his own mind in regard to it as the author of the paper apparently was. If his theory was correct, he could hardly understand why the operation for the removal of the ovaries should not be followed by immediate relief. In the particular case which he had related, the improvement noted was not greater than often occurred spontaneously. Troubles such as he had described were liable to occur without any disease of the ovaries whatever. As to the author's vaso-motor explanation, it was theoretically very pretty; but the fact unfortunately remained that in reality we knew nothing at all about the matter. Dynamic conditions, in his opinion, offered a more satisfactory explanation. He then mentioned a case of his own, which, he said, had taught him a lesson, and which had rendered him very conservative in regard to Battey's operation. In great part, in deference to the opinion of eminent gynecologists, he gave his consent to having the operation performed in this instance. One ovary was removed, and, on examination, it was found to be but little diseased; but, in five days afterward, the patient died, without there having been the slightest change in any of the symptoms. In conclusion, he mentioned a case of hysteria and hystero-epilepsy, at the Breslau clinic, in which a cure was effected by the performance of a sham operation, the patient firmly believing that the ovaries had been actually removed.

DR. C. K. MILLS, of Philadelphia, stated that, in his opinion, in the vast majority of cases operated upon, relief might have been afforded without resorting to this extreme measure. All, he thought, had seen instances of hysteria, with symptoms as severe as those recorded in Dr. Walton's patient, in which this was the case. He then mentioned a case of hystero-epilepsy in which the clitoris and both ovaries were removed, and yet in which, notwithstanding the extent of the operation performed, the patient admitted that she had the same inclinations, and suffered from many of the same symptoms as before, while her mental condition, he thought, was worse in many respects. He believed that we should hesitate very long before recommending an operation of this kind for the relief of hysteria, since, in his opinion, hysteria, except in very rare instances, was not dependent on uterine or ovarian disease. The tendency to operate was very great at the present day, and in two instances that he knew of in which an operation was recommended, the functions of the ovaries had never been established. It would have been much better in these cases, he thought, to endeavor, by every possible means, to secure the establishment of these functions.

DR. J. P. PUTNAM, of Boston, remarked that he could not say that he subscribed to the theory of Dr. Walton; but, at the same time, he could not say why, in certain cases, the operation was not justifiable, just as it was acknowledged to be proper to remove other sources of peripheral irritation. Thus, eye-strain, although he thought its importance in the causation had been exaggerated, was sometimes one of the factors in the production of cerebral disease, and in a certain number of cases the relief of the eye-strain was followed by some amelioration of the trouble.

DR. ROCKWELL said that, in his position, in connection with the New York State Woman's Hospital, he had had ample opportunities for observation in this class of cases, and that he could not but acknowledge that in this institution the operation was performed, as a rule, only in those in which it was proper that it should be. He was convinced, however, that there was a considerable minority of cases in which symptoms were attributed to disease of the ovaries which were not in reality referable to this cause. In the Woman's Hospital, he had often noticed that hysterical, neuralgic, and abnormal sensory symptoms were completely relieved, while ovarian and uterine disease, of which the patient was the subject, remained in the same condition as before. He knew of at least one case also in which it had been proposed to remove the ovaries in which he had himself succeeded in securing a perfect cure by other means. Great care, he thought, therefore, should be used in the selection of cases for operation.

DR. MARY PUTNAM JACOBI, although not a member, having been invited to participate in the discussion, related two or three cases which had come under her observation, and made the point that whenever it could be shown that the ovaries were enlarged and prolapsed, and that they had resisted various methods adopted to restore them to their normal condition, it could be inferred that there was cystic degeneration of these organs. When cystic degeneration was determined to be present, it was only a question of time when they should be removed, for this procedure then became a positive necessity unless, as occasionally occurred, the ovary became converted into one large cyst; in which case it constituted an indolent tumor which gave rise to very little pain or other disturbance. In one case of this latter kind, with which she was acquainted, the patient had no trouble at all, except moderate dysmenorrhœa. On the other hand, there were cases in which there was no objective disease of the ovaries in which there were still some hysterical and neuralgic symptoms. It was this class of cases in which it was entirely questionable whether the operation would afford any relief, even in instances of the most violent pelvic neuralgia. In many cases, while removal of the ovaries put a stop to menstruation, it did not stop the pain; the neuralgic condition remaining the same as before. She did not know whether it had ever been suggested that the pain in cerebral hysteria (that is in cases in which the trouble could be referred to no local cause) was precisely analogous to the aura of epilepsy, which was supposed to indicate that certain changes were taking place in the cortex of the brain.

DR. WALTON said, in closing the discussion, that hardly anyone could be more conservative than himself in regard to the operation, unless it was some one who thought that this procedure ought never to be resorted to; since it was only in cases in which the hysterical symptoms were obviously secondary to pelvic disease that he would recommend it. To the objection that removal of the ovaries did not always cause immediate relief he would reply, that there were many cases in general practice in which a secondary condition was not relieved by the removal of the original cause, as, for instance, the extraction sometimes of a tooth whose condition had given rise to facial neuralgia. One cause why the relief had not been greater in the case which he

had reported, he thought, was that the Fallopian tubes had been allowed to remain (the operation performed being Battey's instead of Tait's). Tait had repeatedly found that menstruation was very apt to continue unless the tubes, as well as the ovaries, were removed. Still, but a comparatively short time had as yet elapsed since the operation, and he trusted that the improvement would become greater as this period extended. He was quite willing to accede to the proposition that all these troubles might exist without there being any disease of the ovaries, and on this account he thought that the greatest possible caution should be observed in advising operative interference. If cases could be relieved without operation, he thought that all other means should be employed, and that in every instance the operation should be reserved as the last resort. As to hysteria *alone*, it was rare that one was called upon to advise operation for its relief. He agreed with those who believed that hysteria was almost always of cerebral origin, and that it was only in a comparatively few cases in which it was dependent on uterine or ovarian conditions. To sum up, he was in favor of operating only in those cases in which we were reasonably sure that organic disease of the ovaries was present.

DR. JAMES J. PUTNAM, of Boston, read a paper on

TYPICAL HYSTERICAL SYMPTOMS IN MEN, DUE TO INJURY, AND THEIR MEDICO-LEGAL SIGNIFICANCE,

during the course of which he related a case of railway injury followed by hysteria, and stated that in a medico-legal aspect, at least, it was very unfortunate that there should not be some standard of opinion established in regard to this subject on account of the confusion which now existed, and for which Erichsen was, to a considerable extent, responsible. The medical opinion in reference to hysteria was, as a rule, very different from that existing in the public mind, and the views of authorities like Erichsen and Page were completely opposed to each other.

DR. DANA said that he was particularly interested in this subject, as within a recent period he had been called upon for an opinion in two cases, in the one on behalf of the patient and in the other on behalf of a railroad company that was being sued for damages in consequence of injuries inflicted on a passenger. He therefore fully appreciated the difficulties to which Dr. Putnam had alluded. There was one point, however, to which he had not referred. Hysteria was, no doubt, a serious disease; but the question arose in every case such as he had described, whether the hysterical symptoms were due to the accident or were excited by the hope of reward. There are two classes of cases in which hysteria was liable after accidents. The first was where serious injuries were received, and the patient suffered immediately. The second was of two kinds: (1) those in which symptoms slowly developed, as in chronic meningo-myelitis, and (2) those in which there might be spinal concussion, followed later by hysteria or hypochondriasis. He had not found the existence of hemianæsthesia in men, as Dr. Putnam had. There was one objective symptom which might perhaps be of some value, and that was electric resistance.

DR. PUTNAM thought that this whole subject ought to be regarded from a different point of view than that from which it was now regarded. The slowness of the

injury did not seem to be of much consequence. In his own case the injury had been slight, and yet from the very first day he had been seriously affected. No thought of obtaining damages, he believed, could have had any effect in originating the hysterical trouble in this instance. He advised that sensibility should be tested in all respects for hemianæsthesia; the spécial senses as well as the skin. The question of atrophy was interesting in this connection. Flattening of the muscles, accompanied with a somewhat diminished temperature would be proof that the parts claimed to be paralyzed had been kept in one position for a long time, and the presumption would be, therefore, that the individual was not feigning. Electrical resistance, he argued with Dr. Dana, was an important point.

DR. SARAH J. MCNUTT, of New York, reported a case of

DOUBLE HEMIPLEGIA.

The patient was a child who had never walked or talked, and who died of broncho-pneumonia, with convulsions. The disease of the brain from which it suffered was attributed to injury to the head received at the time of birth. At the autopsy marked atrophy of the cortex (greater in the right than in the left hemisphere) was found on both sides, involving principally the paracentral lobule, the anterior central convolutions and the upper two-thirds of the posterior central convolution. The disease was pronounced by Prof. William H. Welch to be sclerosis of the greater part of the motor tract, with degeneration of the pyramidal tracts. There were no lesions in the vicinity of the island of Reil, but in the larynx there were found vegetations on both vocal cords, consisting of the natural tissue of the part.

EVENING SESSION.

On the unanimous recommendation of the Council, Dr. Augusta Foul, Professor of Psychiatry in the University of Zurich, Switzerland, who had been nominated by Drs. Seguin and Ott, was elected

AN ASSOCIATE MEMBER.

DR. W. R. BIRDSALL, of New York, read a paper on

OPHTHALMOPLEGIA EXTERNA PROGRESSIVA,

in which he reported two cases of slowly progressive paresis of all the external muscles of both eyes (the levator palpebrarum, the recti, and the obliqui) producing partial ptosis and nearly complete immobility of the eyeballs, with complete preservation of the functions of the internal ocular muscles (iris and ciliary muscles), accommodation being normal, and reaction of the iris to light and accommodation movements also normal. There was no perceptible lesion of the fundus. Vision was normal in one case, and defective from irregular astigmatism in the other. There was no evidence of disease in any other cranial nerve or in any part of the body. There was no heredity, and no signs or suspicion of syphilis. The patients were both males, aged respectively seventeen and twenty-nine. Slow improvement occurred in one case under large doses of iodide of potassium, and in the other, under the use of the same drug and faradization of the eyes. One case was referred to him from the hospital service of Prof. D. B. St. John Roosa, and the other by Dr. Wilson, of Bridgeport, Conn. Both cases were still under treatment.

Dr. Birdsall held that the lesion could not be due to intraorbital disease, nor to an intracranial lesion involving primarily the trunks of the nerves implicated, on account of the escape of those branches of the third nerves which supply the iris and ciliary muscles; and that it must therefore be an affection of the nuclei of the sixth, fourth, and parts of the third nerves, which supply the external ocular muscles—these parts representing an associated system concerned in movements of the eyeballs and in lifting the upper lids, somewhat distinct from the iris and ciliary muscles, which are more intimately connected with the functions of the optic nerve in the regulation of light, and are probably supplied, as far as the sphincter iridis and the ciliary muscles are concerned, from nuclei in close proximity to, but distinct from the nuclei of origin of the remaining third nerve-fibres, which supply the external ocular muscles. A gross lesion, such as a neoplasm, meningitis, arteritis, or a local softening, could hardly affect so widely separated nuclei as the third, fourth, and sixth nerves, and not at the same time affect the ciliary and pupillary centres, those of the fifth and seventh nerves, or other neural tracts. Degeneration within this system of associated muscles, nerve tracts, and centres, similar to the degeneration of progressive muscular atrophy and of labio-glosso-pharyngeal paralysis, appeared to be the most consistent theory of the pathology of these cases.

Dr. Birdsall stated that Hutchinson, in 1869, had advanced this view, based on an atrophy in which this lesion was found by Dr. Gower, to account for this class of cases, which Von Graefe first called attention to. In most of Hutchinson's cases (which were seventeen in number), however, the iris and ciliary muscles were also affected. Other portions of the nervous system, in addition, were implicated in all his cases, so that none of them were exactly parallel to the two now presented, which were apparently unique. Dr. Roosa had exhibited them at the New York Ophthalmological Society, and none of those present had ever met with similar cases, with the exception of Dr. Kipp, of Newark, N. J., who stated that he had seen one case which he thought was identical. This case, however, had never been published. In conclusion, Dr. Birdsall referred to some other allied cases reported by different authors, and spoke of certain relations between tabes dorsalis and progressive muscular atrophy, as well as of syphilitic ocular affections.

DR. SPITZKA thought that Dr. Birdsall's explanation of these cases was probably the correct one. Such troubles were analogous to bulbar paralysis; but there was one difficulty about accounting for them which did not exist in the case of the latter, and that was the great distance between the nuclei of the fourth and the sixth pairs of nerves.

DR. BIRDSALL said that he looked upon the parts mentioned in his paper as associated functionally, and that was the view adopted by Hutchinson. This paralysis, he thought, was a systemic affection, and the reason that he felt obliged to adopt the theory of degeneration was, the greater difficulty of accounting for the trouble by a gross lesion, on account of this very distance between the nuclei of the different nerves involved.

LOCOMOTOR ATAXIA AND SYPHILIS.

DR. E. C. SEGUIN had expected to contribute a paper entitled "A Study of the Etiology of Tabes Dorsalis

with Special Reference to the Efficiency of Syphilis," but was prevented from doing so, as well as from being present at the meeting, by illness. He therefore merely sent some statistics of his cases, which were now read by Dr. Birdsall. There were 72 cases in all; and of these, 22, or 30 per cent., had had chancre, and 16, or 22.2 per cent., chancre followed by secondary symptoms. In 14 cases, or 19.8 per cent., there had been neither chancre nor secondary symptoms, and in 20, or 28 per cent., there was no mention of syphilis. Dr. Birdsall said that in 43 cases seen by him, 9.5 per cent. had had syphilis. His patients had been mostly hospital cases, while Dr. Seguin's had been mostly in private practice.

DR. SPITZKA said that before 1881 he had seen 61 cases of locomotor ataxia, and since 1881, 23 cases. Of the latter series, in which greater care had been taken to secure accurate histories, over 80 per cent. of the patients had had syphilis. But, what he thought was of more value than any figures, he was able to report one case of complete cure by vigorous antisyphilitic treatment. It was a typical case of ataxia (which had at one time been so great that the patient was confined to bed), with the Argyll-Robertson pupil and other pathognomonic signs of the disease; and now it was perfectly cured, with the exception that there was a slight asymmetry in the patellar tendon reflex (which had been completely lost), this being a little fuller than normal on one side, and a little less than normal on the other.

DR. ROCKWELL had seen forty-four cases, all in private practice, and seventeen of these were syphilitic. Of the forty-four, six were women, and two of these were syphilitic. He had no doubt that a much larger proportion of his cases than seventeen had really had syphilis, if the truth were known, and it was his firm conviction that syphilis existed in a majority of all the cases of ataxia, and was by far the most important factor in the etiology of the disease.

DR. PUTNAM had seen 34 cases, and of these about fifty per cent. were syphilitic. In none of the latter, however, had anti-syphilitic treatment been successful.

DR. LEONARD WEBER, of New York, repeated the statistics which he had given in his paper read recently before the Academy of Medicine. These, he said, would seem to indicate that syphilis played quite an important rôle in the etiology of ataxia; but by no means so important as claimed by Erb, Fournier, and others. He was not so sanguine in regard to antisyphilitic treatment as some others. In his experience, when there was a rapid development of tabes in the course of syphilis (the ataxia being due to multiple lesions, probably in the lepto-meninges, and not to true sclerosis of the cord), there was a fair prospect of securing relief; but in the more slowly developing cases of tabes, in which the patient had had syphilis twenty years before, he had yet to see the first case in which any improvement took place. The great expectations of success which had been raised by the announcement of the syphilitic origin of ataxia by Erb and Fournier, he thought, had been by no means realized.

DR. C. L. DANA, of New York, read a paper entitled

FOLIE DU DOUTE AND MYSOPHOBIA.

He referred to the history of *folie du doute*, or the "doubting madness," which had been chiefly studied

by the French writers. Griesinger and Berger had also described a psychopathic symptom which belonged to the same category and had called it *Grübelnsucht*, or metaphysical mania. In *folie du doute* the patient was harassed with a constant desire to question, speculate, and repine over details. His mind was never settled, but was in a condition of "pruritus." It was a psychopathic symptom, which indicated that the patient was a monomaniac and truly insane, or only hypochondriacal, or profoundly neurasthenic and hysterical. It was the form in which *primäre Verrücktheit* sometimes developed. Mysophobia, or fear of contamination, was a symptom indicating probably a similar condition. *Folie du doute* had been divided by Ball into several varieties, such as the suspicious, the calculating, the timid, and finally *tactile*, and mysophobia had been classed as the tactile form of *folie du doute*. This classification was open to criticism, but Dr. Dana was inclined to think it was correct.

He then related two cases, one a typical one of *folie du doute*, as described by Falret, Ball, and others, and the other one of mysophobia, as first described by Hammond. The first patient was a married man of thirty, of a phthisical, but not neurotic, family history, who had indulged in much sexual excess. He was tormented and worried continually as to the ordinary details of his daily life. He had to wash, shave, eat, drink, exercise, work, etc., in a particular way or else he was wretched. The trouble was of ten years standing. Medicinal treatment was tried, but did not have the slightest effect; under a system of moral training and education considerable improvement had been secured, and in his efforts in this direction Dr. Dana had been ably seconded by the man's wife, who was a sensible and intelligent woman. Notwithstanding his infirmity, the patient had always been able to discharge with ability his business duties, which were those of a bookkeeper.

The second patient was a single woman of twenty-eight, of healthy family history, but of hysterical temperament, who for six months had suffered from fear of dirt and contamination, exhibiting the typical symptoms described in cases reported by Hammond, Shaw, Seguin, and Russell. She had never had convulsions or chorea, and was free from uterine disease. Dr. Dana had seen her only once, in consultation, and she did not seem to be very anæmic. She presented the appearance of a hysterical woman, and he also noticed that she seemed to have been the subject of facial paralysis. When she protruded the tongue it deviated decidedly to one side. Under the use of ferruginous and other tonics, as well as of moderate doses of the bromides, she had already improved to some extent, and he had advised that moral influences should be brought to bear upon her a little more forcibly than had hitherto been the case.

In conclusion, Dr. Dana suggested as points for discussion the relation of mysophobia to *folie du doute* and *Grübelnsucht*, and the question of the proper interpretation of morbid fears in general.

DR. SPITZKA said that the class of patients described by Dr. Dana, although of perfectly sound mind in all other respects, were suffering from *bona fide* insanity. In male patients, the one great cause was masturbation. The treatment should be both medicinal and by moral

means, and was best conducted in a small country home, where eight or ten patients were received. He suggested *imperative conception* as a much better term than morbid fears.

DR. G. BETTON MASSEY, of Philadelphia, read the report of

A CASE OF SUDDEN LOSS OF VISION FOLLOWING ANÆSTHESIA OF THE FIFTH NERVE, WITH REMARKS ON THE MODIFYING EFFECT OF ANÆSTHESIA ON THE GALVANO-REACTIONS OF THE SPECIAL SENSES.

The patient was a farmer, forty-eight years of age, who three years before coming under observation noticed numbness of the left side of the face. This increased steadily for about two years, and was accompanied by heaviness and pain in the head, felt mostly on the left side. At the end of that time vision became impaired in the left eye, and in ten days was totally lost. Ophthalmoscopic examination by Dr. William Thomson at this time showed white atrophy, the disk being snow-white. He was then placed under the care of Dr. S. Weir Mitchell, who gave him increasing doses of iodide of potassium, and later the bichloride of mercury in small doses. One year afterwards, on the 8th of February, 1884, he found the right eye failing, and in one week it was entirely blind. Examination showed no light-perception in either eye. Both pupils were very small, and both upper eyelids were kept up by forced effort. The ophthalmoscope showed no decided change except, perhaps, overfulness of the retinal vessels. No cause could be assigned for the attack.

The patient was now referred to Dr. Massey for treatment. Over the left eye there was a partially anæsthetic spot, about four centimetres long, by three wide, oval in outline, and extending laterally. Electrical excitation of this area by placing both poles within its limits, developed the following facts: Faradic current, sensation lessened; galvanic current, local effect on blood-vessels of skin normal. The local galvanic sensations, however, presented a decided deviation from the normal—the sensation of burning being normal, but the sensation of tingling about two-thirds lessened. Having mentioned his observations in regard to gustation, vertigo, and vision in the case, Dr. Massey stated that, at the present time, the patient had been for two months under a treatment consisting of galvanic interruptions to the eyes and numb spot, and increasing doses of sulphate of strychnia, and that as an apparent result there had been a decided increase of sensation in the left brow, and occasional flashes of light in the eyes.

Fearing that the remarkable lessening of normal vertigo and taste originally found in this case might be due to an increased resistance at the anæsthetic spot, he carefully estimated the amount of current actually passing through the circuit by the deflection of a galvanometer needle, and found that this deflection was exactly the same for each side. The amount of electricity passing, therefore, could not have been different, and the difference in the results on the two sides could only be explained by placing these phenomena in the already large list of reflex actions—the integrity of one side of the arc in this case being impaired, with a corresponding impairment of the phenomena. This solution of the "electric taste" and the "electric flash" had been suggested some years ago by Althaus, in his

work on "Electro-therapeutics." Besides a corroboration of the facts observed by him, the importance of the present case, Dr. Massey thought, consisted in the clear relation established between the restoration of sensation and the return of the phenomena.

DR. GEORGE W. JACOBY, of New York, had prepared a paper on

CEREBRO-SPINAL SATURNISM,

but owing to the lateness of the hour, he did not read it. Instead of this, he reported briefly one of the cases described in it, which was one of locomotor ataxia, which he believed to be due to lead-poisoning, and which recovered under the use of iodide of potassium, with the exception that the tendon reflex was not restored. The literature on this subject, he said, was extremely meagre, and he had been able to find only three other cases mentioned.

DR. AMIDON could not see why lead-poisoning should not be a cause of ataxia as well as arsenical and mercurial poisoning and diphtheria.

DR. BIRDSALL said that he had often noticed in patients suffering from lead-poisoning a tremor which amounted almost to ataxia, but he had never seen one with real ataxia of the lower extremities.

DR. DANA thought that, as lead-poisoning was so very common, ataxia would be oftener noticed in connection with it if it were one of the causes of that disease, and that it would require a considerable amount of evidence to prove that Dr. Jacobi's case was really due to this cause.

The Association then adjourned.

RHODE ISLAND MEDICAL SOCIETY.

Annual Meeting, held at Providence, June 19, 1884.

(Specially reported for THE MEDICAL NEWS.)

THE Society met in Lyceum Hall, Providence, THE PRESIDENT, DR. JOB KENYON, in the Chair.

DR. GEO. D. HERSEY read

THE SECRETARY'S ANNUAL REPORT,

which stated that the Society had held four meetings during the past year; that four deaths had occurred among the Fellows, two had removed from the State. The present active membership is 187, nine new Fellows having been received during the year.

DR. C. W. PARSONS read the report of the trustees of

THE FISKE PRIZE FUND.

The sum of \$300 has recently been awarded to Dr. Chas. V. Chapin, of Providence, for the best essay on "The Origin and Progress of the Malarial Fever now Prevalent in New England." No award was made on the other proposed subject.

For the year 1885 the following subjects are submitted, for the best essay on either of which, a prize of two hundred (\$200) dollars is offered:

- (1) "Original Investigations in Household Hygiene."
- (2) "The Present State of the Germ Theory of Disease."
- (3) "Physiological and Pathological Effects of the Use of Tobacco."
- (4) "Migraine, its Nature and Treatment."

THE REPORT OF THE BOARD OF CENSORS

was presented by Dr. J. W. C. Ely. The following gentlemen were recommended for Fellowship and subsequently elected: Drs. Henry S. Swan, William A. Tremaine, Byron J. Lillibridge, Sanford S. Burton, Benjamin R. Symonds and Abraham L. Falcon. The Board also recommended Dr. Lloyd Morton, of Pawtucket, for Anniversary Chairman for 1885.

THE REPORT OF THE TREASURER,

DR. C. H. LEONARD, showed a balance of cash on hand of \$218.88. Upon motion of Dr. Hersey, it was voted to transfer \$200 of this balance to the printing fund, which would then amount to \$1733.43.

DR. T. NEWELL reported for

THE LIBRARY COMMITTEE

that there were 3586 volumes in the library; showing an increase of 1083 volumes for the year. The invested building fund now amounts to \$542.90. Dr. H. G. Miller, of the same committee, said that the Society had been remarkably favored in securing at a reasonable price seven hundred volumes from the private library of the late Dr. David King, of Newport, the purchase having been made at the auction sale in New York. Many of these volumes were exceedingly choice and valuable.

DEATH OF DRs. PERRY AND BULLOCK.

Dr. W. E. Anthony, of a committee embracing also Drs. J. H. Eldredge and A. Ballou, presented the following preamble and resolutions, which were adopted.

Whereas, In the decease of Drs. Otis Bullock and Thomas W. Perry this Society has lost two members who had long been connected with it;

Resolved, That we desire to place on record our appreciation of those traits of character by which they had attained such a position of usefulness and honor in the Society and in the community. Dr. Bullock was for one year President of the Society, and for forty years was a member of the Board of Censors, and, as such, conscientiously fulfilled all the duties pertaining to the office with unfailing kindness and courtesy. Dr. Perry was a frequent attendant on our meetings, and often contributed the result of his large experience to the discussions.

DR. LLOYD MORTON, Chairman of the Committee on THE LEGISLATIVE BILL FOR THE APPOINTMENT, ETC., OF MEDICAL EXAMINERS,

reported that the bill, in a considerably modified form, had already become a law, and would go into effect July 1st, proximo. It was then voted to accept this report, and to discharge the Committee, with the thanks of the Society for their efficient services.

OFFICERS FOR THE NEXT YEAR

were then elected as follows:

President.—Oliver C. Wiggin, M.D., of Providence.

Vice-Presidents.—H. G. Miller, M.D., of Providence; John W. Sawyer, M.D., of Providence.

Recording Secretary.—George D. Hersey, M.D., of Providence.

Corresponding Secretary.—E. M. Harris, M.D., of Providence.

Treasurer.—Chas. H. Leonard, M.D., of Providence.

Board of Censors.—Drs. Ariel Ballou, James H. Eld-

redge, J. W. C. Ely, George P. Baker, S. S. Keene, Benjamin Greene, Edward T. Caswell, and Eugene Kingman.

It was voted to hold the *September meeting* of the Society in Providence.

THE ACTION OF QUININE.

DR. GARVIN, of Lonsdale, said that in view of the very mistaken and, in many instances, harmful, impression that exists in the minds of many people regarding the effects of quinine on the system, he wished to offer the following:

"*Resolved*, that the use of quinine in the treatment of intermittent fever, leads to no injurious after-effects."

In the course of a brief discussion on the resolution, Dr. A. Ballou said it seemed to him to be hardly becoming the dignity of the Society to take such action. In this sentiment the Society evidently concurred, as the resolution was not adopted.

The *Annual Address* was then delivered by Prof. Geo. I. Chace, of Providence, after which the Chair called upon the President-elect, Dr. Wiggin, who made a brief but graceful speech of appreciation of the honor conferred on him.

The Society then adjourned to the Narragansett Hotel to dine, Dr. J. W. C. Ely acting as Anniversary Chairman.

KENTUCKY STATE MEDICAL SOCIETY.

Thirty-ninth Annual Meeting, held at Bowling Green, June 3, 4, and 5, 1884.

(Specially reported for THE MEDICAL NEWS.)

(Concluded from page 713.)

WEDNESDAY, JUNE 4TH—SECOND DAY.

AFTERNOON SESSION.

DR. PINKNEY THOMPSON, of Henderson, read a paper on

TYPHOID FEVER,

which, he said, is a disease that has a distinct clinical history and runs a distinct course, and in his judgment is never aborted by remedies. The paper elicited considerable discussion in reference to treatment.

DR. A. W. JOHNSTONE, of Danville, read a paper on *Litholapaxy*.

DR. DUDLEY S. REYNOLDS then spoke on *Inflammations of the Iris*.

THURSDAY, JUNE 5TH—THIRD DAY.

DR. S. D. FINCK, of Louisville, read a paper on *Phlyctenular Conjunctivitis*.

DR. W. M. FUQUA, of Hopkinsville, read an elaborate paper on *Civilization and Sanitation*.

DR. WM. CHEATHAM, of Louisville, read a paper upon

SYMPATHETIC OPHTHALMIA,

in which he presented the following conclusions: Enucleation is better performed in the stage of irritation; if it cannot be performed then, operate in the stage of inflammation, for while there is life in the eye there is hope. It should not be performed in iritis serosa. It can be performed at the onset of irido-cyclitis, although

the eye causing the sympathetic inflammation may not be blind.

The following resolution was adopted on the

DEATH OF DR. SAMUEL D. GROSS.

Whereas, the late Dr. Samuel D. Gross, of Philadelphia, was one of the earliest Presidents of this Society, and always on its rolls, it is fit that we should formally add our meed of respect to his memory.

What we shall say of him is much to us, though it is but a small part of the large sum of testimony to his worth going up from medical men in both hemispheres. In recognizing his high merits we shall join company with the most dignified and venerable institutions of learning known to the English-speaking race; institutions that have done themselves honor in bestowing honors upon him. It is some comfort to us who mourn him, that in the corporate existence of this organization, of which he was a founder, *he still lives*. To our remotest year his name shall stand as that of one upon whose full and excellent life every member may well wish his own were modelled.

Resolved, That as he was a great surgeon and leader of thought we shall continue to hold his precepts in high esteem. As he was a kind and loyal friend we shall cherish our memories of his personal dealings with us; as he lived frankly, purely, and courteously, we shall reverence in him the gentleman above reproach.

DR. S. H. STUCKY, of Louisville, read a paper on *Catarrhal Headache and Its Allied Affections*.

DR. J. M. MATHEWS, of Louisville, read a paper on

OPERATIONS UPON HEMORRHOIDS DURING THE INFLAMED STATE.

He held that a pile is a tumor, and he took issue with those authorities who characterized this tumor as a vascular tumor. He maintained that it is a tumor which is well supplied with blood; the vessel which feeds it can be felt at the top pulsating with a beat almost equal to that of the radial artery. He defined three varieties of these tumors: First, the capillary, in which the small vessels push into the folds of the mucous membrane, without any infiltration at all. That is one kind; and it is a dangerous kind from the fact that the circulation is free and rapid. Being fed by an artery of considerable size, if ulceration takes place over it and perforate it, there is danger that the patient will bleed to death. If the chain of morbid phenomena be not here checked, and the inflammation is allowed to continue, it goes on to the formation of the second variety, the venous pile. Admitting the existence of piles, he asked, can the necessity of an operation be disputed? And he took issue with any one who claims to cure this kind of pile or tumor by anything else than an operation. From a variety of causes piles are liable to become inflamed, and, once inflamed, they may easily become strangulated by passage below the sphincter; everything is aggravated then, and it may take some weeks to quiet this trouble. It has occurred to him, why not operate upon them, and get rid of them at once. There is no authority that says, operate upon a pile during the inflamed state; but they will tell you to apply treatment to reduce the inflammation.

He then cited the following cases. A few weeks ago

he was called in consultation to a distinguished lawyer, who was in this condition. The family physician in attendance had tried in vain to quiet inflammatory action for two or three weeks. He found hanging down from the anus two solid tumors; he passed the knife around them, and ligated them. He visited him the following morning, expecting to find him in some trouble. To his surprise and satisfaction, he was out of bed, and, in a week, out of the house.

Another case, a young man, had a mass of inflamed tissue hanging from him as large as his fist, which it would have taken weeks to abate. He ligated the whole mass. He went to see him the next morning, and was told that he had risen early and gone out. Three days later, he received a postal from Cairo, Ill., saying that his patient was that far on his way home, and was all right. When he got home, he wrote that he was entirely well. Since then, Dr. Mathews has had five or six cases of similar character, in which the proceedings and results were similar. He therefore concludes that, instead of applying remedies to relieve the inflammatory trouble, the tumors should be operated upon at once. The Society then adjourned.

NEWS ITEMS.

LONDON.

(From our Special Correspondent.)

THE OPHTHALMOLOGICAL SOCIETY OF THE UNITED KINGDOM met on June 5, and transacted some important scientific business.

PREVENTION OF BLINDNESS FROM OPHTHALMIA NEONATORUM.—DR. BRAILEY read the report of the Committee on this subject. In answer to a very large number of inquiries issued to private persons, ophthalmic and lying-in hospitals and institutions for the blind, twenty-three statistical replies, all of them from institutions for the blind, were received. Four of these, those of the Belfast Deaf, Dumb, and Blind Institution, the London Society for Teaching the Blind to Read, the Blind School at York, and the Blind Institution at Hull, were tolerably explicit, and the answers appeared from other evidence to be trustworthy. In the Belfast Institution, thirty per cent. of the persons concerned, owed their blindness to ophthalmia neonatorum. In the London Society, twenty per cent. were certainly blind from this disease; but from the extremely frequent occurrence of "congenital cataract" and "cause unknown" in cases of blindness from birth, the Committee were of opinion that at least another ten per cent. should be added, thus making again thirty per cent. In the Yorkshire School, forty-one per cent. were blind from this cause; and in the School at Hull, five cases out of fourteen (thirty-five per cent.), personally examined by Dr. Rockliffe, were with certainty attributed to the same disease. The statistics of the other institutions were inexplicit, but pointed to about the same results. The numbers agreed substantially with those of foreign investigators, notably those of Reinhard, who, on investigation of twenty-two German blind asylums, found six hundred and fifty-eight blind from this disease among a total of twenty-one hundred and sixty-five (thirty and a half per cent.).

The Committee recommended the adoption by the

Ophthalmological Society of the following resolutions (modified from those originally suggested by Dr. David McKeown, of Manchester):

1. That the purulent ophthalmia of newborn infants being the cause of a vast amount of blindness, mainly because of the ignorance of the public regarding its dangerous character and the consequent neglect to apply for timely medical aid, it is desirable to instruct those in charge of newborn children by a card, in substance as follows: "Instructions regarding newborn infants. If the child's eyelids become red and swollen, or begin to run with matter, within a few days after birth, it is to be taken without a day's delay to a doctor. The disease is very dangerous, and if not at once treated, may destroy the sight of both eyes." This is to be distributed through the medium of the Poor Law and Birth Registration Organization of the United Kingdom.

2. That the advocacy and aid of the medical press be solicited in drawing general attention and especially that of the authors of text-books of midwifery, of the lecturers on the same subject for students and midwives, and of the various institutions which train, and charitable institutions which employ, midwives, to this important subject.

3. That a copy of the first resolution be forwarded to the respective Presidents of the Governing Boards of the United Kingdom.

The scientific affairs of the evening next occupied attention. It may be mentioned that Herren Becker, of Heidelberg, and Snellen, of Utrecht, were present.

MONOCULAR DIPLOPIA.—DR. BRAILEY communicated a case of this disease in a man aged fifty-six. He had paralysis of the right external rectus with dilatation of the pupil. The diplopia only existed in the outer half of the field of vision. The optic disks had subsequently atrophied. Red and green colors could not be perceived. The right knee-jerk was almost absent. The patient had had syphilis.

TUBERCLE OF THE CHOROID.—DR. MULES showed drawings and read a paper on this subject. The patient was a girl, aged ten, who died of acute miliary tuberculosis fourteen days after the apparent commencement of her illness. There was double optic neuritis, and surrounding the disk in each eye were eight or ten circular nodules, yellowish-white at the centre and passing gradually at the periphery into the color of the natural choroid.

SEROUS CYST OF THE IRIS.—DR. ADAMS FROST exhibited a case in a man aged twenty-eight. Twenty-four years before, the eye had been injured by a fork, and the development of the cyst dated from that time. Ten years ago an iridectomy had been performed. The cyst occupied the upper and inner quadrant of the anterior chamber. It would be difficult to remove the cyst, as it was in contact with the cornea and lens.

SARCOMA OF THE CHOROID.—MESSRS. COWELL AND JULER exhibited a glycerine jelly preparation and microscopic sections of a case of sarcoma of the choroid.

DISSEMINATED CHOROIDITIS.—MESSRS. CRITCHETT AND JULER exhibited a living case of disseminated choroiditis in a woman aged forty-six. There was a distinct history of syphilis acquired five years before. The case was chiefly interesting on account of the great number

of large and small yellowish-white patches distributed over the fundus, including the yellow spot region of the right eye. Very little pigment was to be seen, and the visual acuteness and visual field of the right eye were normal. The left eye had become totally blind three years ago; presumably from the same cause.

MR. JONATHAN HUTCHINSON related a case in which the appearances were precisely similar, and which occurred in a young lady, aged fifteen, who was the subject of congenital syphilis, but there were no other manifestations of this diathesis at the time.

ACUTE OPTIC NEURITIS ASSOCIATED WITH ACUTE MYELITIS.—DR. SHARKEY read a very interesting paper on a case of this disease. The patient, a girl, aged seventeen, lost her sight in four days, November 9 to 13, 1883. There were no other symptoms of any sort. Severe double optic neuritis was found, and thirty-three days after her vision failed she began to complain of paralysis with loss of sensation, first in the left leg and then in the right. Anæsthesia gradually spread up the trunk and involved the arms, and she had incontinence of the evacuations. Cystitis came on, she passed offensive purulent urine, and the temperature reached 105° F. Finally, she died of peritonitis, sixty-two days after the blindness had begun and twenty-nine after the appearance of paralysis. At the necropsy, peritonitis, cystitis, and suppurative nephritis were found. To the naked eye the brain and its membranes were healthy. But there was softening of the cervical region of the spinal cord over a space of two or three inches in length. Microscopic examination revealed the presence of acute inflammation with softened cervical region in the columns of Goll above it, and in the lumbar enlargement, as well as in the optic nerves, disks, chiasma, and tracts, there was slight inflammation of the meninges about the chiasma and on the under surface of the frontal lobes. The etiology was quite obscure. Clifford Allbutt, Seguin, Noyes, Steffen, and Erb, had noticed the association of acute myelitis and optic neuritis, and more recently Dreschfeld and Chisolm. Gowers and Dreschfeld, thought they were associated phenomena due to a common cause. Clinically, such cases are of great importance, as they simulate coarse disease of the brain.

AMAUROSIS IN INFANCY.—MR. NETTLESHIP read a paper on amaurosis in infancy. He dwelt on the question of prognosis, and pointed out that there was a fairly well-defined group of cases in which the amaurosis was not recovered from; sight was absent or defective from birth, several children of the same parents were often affected and the parents were often akin. Some of the patients were idiotic, others stupid or weak, others quite intelligent; the optic disks were either healthy or atrophied; in the milder cases, in which sight was partly retained, there was often total color-blindness; in cases of absolute blindness, the pupils still reacted to light, which appeared to show that the centre for pupillary action in or near the corpora quadrigemina remained intact, although higher centres were absent or atrophied. In cases of amaurosis in which recovery was probable, the history was of great assistance; it was generally stated that the child could see well for some months or even years after birth, and then it lost its sight during an illness with cerebral symptoms often diagnosed as meningitis; the optic disks in the great majority of cases were either

healthy or showed merely some slight doubtful pallor; frequently the power of walking, standing, or even sitting, was lost during the same illness. The blindness appeared to last from one to six months, and recovery took place slowly. He thought that in all these cases intracranial inflammation certainly took place, and perhaps spinal mischief as well. Possibly, in some cases, the blindness might be produced by pressure of fluid accumulated in the ventricles, but the explanation would not serve for all cases.

SANITARY PRECAUTIONS.—The Governor of Florida has forwarded to the Surgeon-General, U. S. Marine-Hospital Service, with his approval, a series of resolutions from the Board of Health of Tampa, Florida, requesting the establishment of a government quarantine station at the mouth of Tampa Bay, with a medical officer in charge, and enforcement of such quarantine regulations as the necessities of the case may require. The Board states that, in making the request, it is "influenced rather by considerations for the public safety than apprehension for the security of the town of Tampa, as the Board of Health reserve the right to, and can and will establish such quarantine by land as well as by water as may be necessary to protect the public health of the town from epidemic diseases should the necessity for so doing arise; but of course feel that an efficient quarantine at the mouth of the Bay would tend greatly to allay apprehension in this city as well as in the various towns and cities on the St. John's River."

A dispatch was sent to the Governor of Florida, informing him that an inspector would be stationed at such point as he might direct to tide over any emergency during the summer, but that regular quarantine buildings could not be built out of the present appropriation.

A telegram signed by the Governor of California, Mayor of San Francisco, and the State Harbor Commissioners, was received at the Treasury Department, stating that "there are no quarantine facilities at the port of San Francisco, the same are urgently needed, and the U. S. Government is earnestly requested to aid in providing the same." Answer was sent that the quarantine fund under control of the Department was not sufficient to establish quarantine buildings and necessary appliances, but that an inspector with boatmen and tents can be placed at such point as the Harbor Commissioners unite upon, to tide over the emergency.

DIPHTHERIA IN CAPE BRETON.—Information has been received, through the Secretary of State, from the U. S. Consul at Pictou, Nova Scotia, stating that diphtheria is so prevalent at several ports in Cape Breton that he has instructed his consular agents to advise masters of vessels bound to the United States to take out bills of health.

YELLOW FEVER IN MEXICO.—The deputy collector at Tucson, Arizona, under date of June 13th, says that yellow fever has reappeared at Guaymas, Mexico, where it prevailed so extensively last year. He adds that the sanitary laws are so lax in Mexico as to render it impossible to obtain authentic information, that burial permits are granted without a medical certificate, and no reliable data can therefore be obtained, but that the

United States Consul at Guaymas is his authority for the reported prevalence of the disease and that there is little room to doubt the Consul's statement both as to its existence and fatal character, although the Mexican authorities are issuing "clean bills of health" to outgoing vessels, a course they pursued last year even after the pestilence assumed epidemic proportions.

THE CHOLERA.—Surgeon-Major McLeod, the health officer of Calcutta, under date of April 30, speaking of the excess of deaths for the month (1460, against 972 for the previous month), says: "This monthly total exceeds all the corresponding figures of the past decade, excepting 1878, and also exceeds the decennial mean by 318. . . . There were 486 deaths from cholera, against 123 the preceding month. The number is very high, and exceeds all the corresponding figures of the past decade. It also surpasses the decennial mean by 275."

Consular reports from Calcutta give the following figures:

Week ending April 26,	174 deaths from cholera.
" " May 3,	106 " " "
" " " 10,	91 " " "

Authentic information has been requested by the State Department from the U. S. Consul at Toulon, relative to the reported outbreak of Asiatic cholera in that city.

THE FIFTH INTERNATIONAL CONGRESS OF HYGIENE will be opened at the Hague on the 21st of August, and will continue in session one week. Applications for membership may be at once made to the General Secretary, Dr. Van Overbeck de Meijer, Professor of Hygiene at the University, Utrecht, Holland. Among the papers announced to be read are one on the "Attenuation of Virus," by M. Pasteur; on "Infectious Diseases," by Dr. Koch; and on the "Practical Application of the Doctrine of Infectious Poisons" by Dr. Von Kelnburg, of Bonn.

HOW TO PREPARE A 1 TO 1000 SOLUTION OF CORROSIVE SUBLIMATE.—SIR JOSEPH LISTER writes to the *British Medical Journal* (May 24, 1884), that one drachm by weight of a solution of one part of corrosive sublimate in one and a half part of glycerine contains two-fifths its weight, or twenty-four grains of the sublimate. This, multiplied by 1000 (the proportion of water required), gives 24,000 grains, which is very nearly three pints. It is, however, much more convenient to use fluid measure than weight, and a fluidrachm of the glycerine solution referred to requires four pints of water to produce the 1 to 1000 solution.

M. PASTEUR.—It is stated that it is proposed to offer M. Pasteur a seat as life senator in the place of Mr. Wurtz, recently deceased. We also learn that the Minister of Public Instruction has obtained for M. Pasteur ground in the Bois de Boulogne, in which to prosecute his experiments with the virus of rabies.

SIR HENRY WENTWORTH ACLAND, K.C.B.—The many friends, on this side of the Atlantic, of Dr. H. W. Acland, Regius Professor of Physics at Oxford, will

be gratified to learn that the Queen has been pleased to confer upon him the dignity of Knight Commander of the Bath.

THE ANTIVIVISECTION AGITATION FROM A LAYMAN'S POINT OF VIEW.—George Augustus Sala, in his "Notes of the Week" in a late number of *The Illustrated London News*, writes:

"The Committee of the Antivivisection Society have sent me a circular referring to a meeting of the members and supporters of the Society to be held on Friday, May 9th.

"I have nothing to say, myself, against vivisection (when placed under proper legal restrictions), for the simple reason that for fifty years I have been in constant contact with the doctors, and have known some of the most eminent surgeons of the last and the present generations.

"I was, in early childhood, that which I once heard my mother's maid, in conversation with a friend, describe me as being 'a miserable little Hobject;' and the doctors were continually doing things to me. I am, in many respects, a miserable object now, and require to be periodically patched up by the faculty. And I will say naught against vivisection, because I never yet met with a cruel doctor; and because I firmly believe medical men to be among the most humane, the most generous, the most unselfish of mankind; and I indignantly denounce the imputation that there are any members of a noble, a high-minded, and a beneficent profession who, actuated by no more exalted motives than their 'greed after the hidden things of the life of the flesh,' confessedly and without compunction torture dumb animals. The medical man practises vivisection (with the smallest possible infliction of pain on the animal vivisected), not because he is selfishly greedy after 'the hidden things of the life of the flesh,' but because he wants to find out what is the matter with *you*, verbose Sir or sentimental Madam; because he is striving his utmost to devise means for saving *you* from misery and anguish; for mitigating the agony of the ills that rack the joints, or fire the veins, or that 'in the deeper vitals rage.' He is no worshipper of knowledge as of an idol. He tries during his whole laborious life to learn more and more, in order that he may console and relieve and preserve suffering humanity."

NEW MEDICAL JOURNAL.—The Italian journals announce the first number of *Commentario Clinico delle Malattie degli Organi Genito-Urinari*, published in Pisa by PROF. BARDUZZI. It is a bimonthly journal of forty pages.

THE ILLINOIS MEDICAL REGISTER.—The Illinois State Board of Health is now engaged in revising the Register of Physicians, preparatory to publication. Any changes or corrections should be promptly sent to the Secretary.

Lists of the Officers of the Medical Societies in the State are also requested.

RESIGNATION OF PROFESSOR GOSSELIN.—Professor Gosselin, the acknowledged head of French surgery since the death of Nélaton, has, by reason of advancing years and impaired health, found it necessary to resign his Chair in the Faculté.

M. PASTEUR has been furnished with an opportunity of testing his theories concerning rabies upon the human subject. One of the servants of the Paris and Lyons Railway at Tarascon-sur-Rhone, having been bitten by an undoubtedly mad dog, has just placed himself in the hands of the illustrious savant.—*Brit. Med. Journ.*, June 14, 1884.

THE S. D. GROSS PROFESSORSHIP OF PATHOLOGICAL ANATOMY.—The Committee acknowledge the receipt of the following subscriptions:

Dr. J. M. Da Costa, Philadelphia,	\$2000
Dr. S. W. Gross, Philadelphia,	1000
Dr. J. Marion Sims, New York,	500
Dr. W. W. Keen, Philadelphia,	100
Dr. J. Ewing Mears, Philadelphia,	100
Dr. Joseph Hearn, Philadelphia,	100
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R. J. DUNGLISON, M.D.,	
Treasurer,	Chairman.

P. O. Lock-box No. 1274, Philadelphia.

OFFICIAL LIST OF CHANGES IN THE STATIONS AND DUTIES OF OFFICERS SERVING IN THE MEDICAL DEPARTMENT, U. S. ARMY, FROM JUNE 17 TO JUNE 23, 1884.

MAGRUDER, D. L., Lieutenant-Colonel and Surgeon.—Granted leave of absence for one month.—*Par. 7, S. O. 143, A. G. O.*, June 20, 1884.

HAPPERSETT, J. C. G., Major and Surgeon.—Granted leave of absence for four months.—*Par. 5, S. O. 141, A. G. O.*, June 18, 1884.

THE MEDICAL NEWS will be pleased to receive early intelligence of local events of general medical interest, or of matters which it is desirable to bring to the notice of the profession.

Local papers containing reports or news items should be marked.

Letters, whether written for publication or private information, must be authenticated by the names and addresses of their writers—of course not necessarily for publication.

All communications relating to the editorial department of the NEWS should be addressed to No. 1004 Walnut Street, Philadelphia.

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